THE DEFINITION OF PSYCHOLOGY

The Century Psychology Series
Richard M. Elliott, Editor

THE DEFINITION OF PSYCHOLOGY

An Introduction to Psychological Systems

By
FRED S. KELLER, PH.D.
COLGATE UNIVERSITY

D. APPLETON-CENTURY COMPANY
Incorporated
NEW YORK LONDON

4106

PRINTED IN THE UNITED STATES OF AMERICA

PREFACE

This book is the partial result of my attempt, during the past six years, to provide college students with an introductory course of instruction in psychology that will serve at once as an orientation for those who expect to go no further into the field and as a pre-view for those who plan to concentrate therein. The material here presented has been selected with the aim of leading the beginner along a portion of the route that psychology's fathers and founders traveled on the way to the modern problem of defining and systematizing the science. In my own teaching practice this material constitutes the first part of the course-work and is followed regularly by an outline treatment of four major fields—animal, child, differential, and abnormal psychology—a text on which is now in preparation. Such a mode of approach is related to my conviction that the usual beginner's text or lecture-course in psychology fails to supply an adequate conception of the developmental aspect of the province or of the breadth and variety of interest and research within it.

It will be difficult for me to repay Dr. B. F. Skinner of the University of Minnesota, Professor Clarence W. Young of Colgate, and my wife, Frances, for their encouragement and aid during the actual composition of these chapters. Without their criticism and the helpful editorial comment of Professor Elliott, many more faults might be found with the present portrayal than are now exposed.

I am grateful to Harcourt, Brace and Company, Henry Holt and Company, J. B. Lippincott Company, Liveright

Publishing Corporation, Longmans, Green and Company, The Macmillan Company, W. W. Norton and Company, and Charles Scribner's Sons for permission to quote passages from their books.

FRED S. KELLER.

Hamilton, New York

CONTENTS

THE DEFINITION OF PSYCHOLOGY

Chapter I

THE HISTORY OF THE PROBLEM

LONG before psychology came to be treated as an experimental science there were men who interested themselves in matters that would now be called psychological. The influence of these men upon later generations has been great, and it is not strange that we should approach the problem of defining modern psychology by reference to their opinions and discoveries. In fact, it is only by so doing that we can properly appreciate the difficulty of defining psychology or evaluate the tremendous advances made in recent years.

Although our historical preparation will be limited to the mention of a mere handful of men, actually there were hundreds—perhaps thousands—whom we might consider as forerunners of present-day psychology. No science goes ahead by the leaps and bounds which an initial excursion into its history seems to indicate. Progress is slow, a fact that should be encouraging to the student who aspires to add his item to the achievement of the past.

In dealing here with the beliefs of these early representative figures, I do not mean to imply that one must subscribe to them, nor do I quote them as authoritative. Some of them would nowadays be termed fantastic—even silly—and most of them are of historical importance only. As a rule, the fact that they were held served merely to bring some problem into the focus of attention, not to solve it to every one's satisfaction.

How far into the past shall we reach to pick up our

historical threads? This is not an easy question to answer. We might begin with the "psychological" views of primitive man, especially with his beliefs concerning the "ghost-soul"; but our factual footing would be none too secure at such a distance. Or we might start with a more articulate age—with Aristotle (384-322 B.C.) of ancient Greece, the true father of all psychology; with Claudius Galen (c. 130-199 A.D.), the Roman physician whose classification of temperaments and localization of reason in the brain foreshadowed much of modern doctrine and research; with Thomas Aquinas (1224-1275), the voice of the medieval church in many psychological matters. None of these, however, is as directly and immediately in line with our present concern as the French philosopher and mathematician, René Descartes (1596-1650)—as exciting a figure, personally and intellectually, as one may hope to uncover in turning the pages of a history book.

Born of the lesser nobility, trained in a Jesuit school, a soldier for a time (by some accounts not above the "excesses of youth"), and, finally, a scholar of high rank and radical opinions—the story of this man's life might easily divert us from the main track of our interest. We must, therefore, content ourselves here with a brief treatment of the reasons which rightfully entitle him to be called the Father of Modern Psychology.

René Descartes was the first great "dualist" among the world's philosophers. He was the first to make a sharp distinction between "mind" and "body"—a distinction which has made no little trouble for psychologists down to the present day. Furthermore, he was that kind of a dualist which we call an "interactionist"—that is, he believed that the mind may affect the body and the body the mind.

Descartes' views were nearly identical with the "common-sense" opinion of most of the persons who will read this account—proof convincing enough of his influence upon the

thought of later generations. The "mind," to Descartes, was that which "thinks"; the principal site of its activity was in the head; and it could take up no physical space. The "body," on the other hand, was a clearly objective "extended substance," mechanical in its action and obeying all the known laws of the inanimate. Animals, indeed, having no minds or souls (the two terms were synonymous to Descartes), were considered to be nothing more than machines.

The hypothesis advanced by Descartes to explain the interaction of mind and body was, if inaccurate, at least ingenious and in some accord with the existing views of the functions of the human nervous system. As an illustration we may consider one phase of this speculation—that which concerns the manner of the mind's influence upon the body.

The sensory nerves of the body are likened by Descartes to bell wires which convey the influence of the external world to the central "cavern" or ventricle of the brain; the motor nerves are taken to be tiny tubes in which "animal spirits" or blood vapors arising from the heart pass from the brain's cavern out to the muscles and there cause bodily movement.* Thus, an excitation of some sense-organ would bring about a tugging of the bell wire, which, at its central termination, is capable of opening minute valves in the ends of near-by motor nerves and permitting a subsequent flow of the spirits to the proper muscles, and ultimately provoking action. This is a very mechanical notion of the nervous system and, at the same time, an anticipation of views which, being much more modern, are much less laughable.

But what of the influence of the mind? Descartes' answer

* "Sensory" and "motor," as the reader may know, are the terms applied respectively to those nerves which carry impulses from the sense-organs to the brain or spinal cord and from the brain or cord to the motor organs, e.g., the muscles.

was direct, if not plausible. His argument was that the soul or mind, being unitary, must influence the body, which has two symmetrical halves, through the agency of some single structure shared by both body-halves. The structure that he chose was the pineal body, a small gland of the brain that, for him, projected into the turbulent vapors of the central cavern. The movement of this structure, at the mind's command, was supposed to be capable of changing the spirit flow and interrupting the bell-wire–tube sequence of activity; the soul's desire was thus translated into bodily motion and interaction was achieved.

Descartes made other contributions, some of which will be mentioned in connection with other problems and the thoughts of other men, but none have proved quite so exasperating theoretically as these general conceptions of dualism and interactionism. We shall see this clearly further along when we consider the views of modern "systematists" in psychology.

John Locke (1632-1704), the British philosopher, became interested in psychological matters through a friendly discussion of the nature and acquisition of knowledge. At that time he undertook to write a very brief statement of his views with respect to this problem. Twenty years later he published the book that resulted from his attempt, *An Essay Concerning Human Understanding*—a book that can still be a source of quiet delight for the unhurried reader.

It is in this book that we find a development of the theme, at that time rather radical, that "all ideas come from experience." Locke compared the mind in its virginal state to a sheet of white paper upon which experience writes. He said: "Let us suppose the mind to be, as we say, white paper, void of all characters, without any ideas; how comes it to be furnished? . . . To this I answer, in one word, from experience."

This view is not really a new one historically. Even Aris-

totle had spoken of the mind as initially a blank tablet (*tabula rasa*). But the development of the view is Locke's own; and it came at a very appropriate time. Descartes, and others, had argued for "innate" ideas—ideas especially clear and belonging to the mind by virtue of no influence of the outside world.

In taking his position, elaborating it, and defending it with page on page of careful analysis and direct appeal to his own experience, John Locke inaugurated a movement in philosophy now known as "English empiricism," which has had such widespread effect that we hardly recognize its presence in our thinking nowadays. Without it, however, the rise of modern observational and experimental psychology, of which we are so proud, might well have been delayed for many years.

An "idea," to Locke, was "whatever it is which the mind can be employed about in thinking." Whiteness, hardness, sweetness, man, motion, drunkenness, elephant, army, and thinking—all these were mentioned by him as typical ideas, and all might be considered to come from one of two sources: directly, from the senses, or indirectly, from the mind's reflection upon the sense-knowledge—ideas, and ideas about ideas!

Furthermore, Locke considered that ideas might be simple or complex, the latter being really compounds of the former and reducible to them, upon careful analysis. Thus, if to the idea of substance "be joined the simple idea of a certain dull whitish colour, with certain degrees of weight, hardness, ductility, and fusibility, we have the idea of lead," a complex idea. In this way Locke threw a bone of contention far into the future, for, as we shall see, the possibility of analyzing the human mind into elements, as well as the probable nature of these elements, has been a hotly debated issue in very recent years. Locke's immediate successors carried these notions of analysis and compound-

ing to regrettable extremes, and the reaction has been vigorous.

Since Locke has often been labeled "the first associationist" it may be well to include here a mention of the fact that the most generally used term to describe the combination and compounding of ideas is "association." Locke himself used the phrase "association of ideas" as a chapter-heading in the essay, but it was left to his followers to give it the stress that has embedded it so firmly in our own everyday vocabularies.

One more distinction made by Locke may serve as a good introduction to the teachings of the next philosopher on our list: the distinction made between what he called the "primary" and "secondary" qualities of objects, which we may treat as a difference between ideas. Briefly stated, it is this: some simple ideas of sense resemble the objects of the external world which cause them; other simple ideas of sense, although caused by such objects, do not resemble them. For example, ideas of solidity, figure, and motion are like the external objects; but ideas of colors, sounds, or tastes are *un*like anything in the objects which arouse them.

We need not bother ourselves with his reasons for dividing simple ideas of sense into these two groups, but it may be pointed out that here was a recognition by Locke that our perceptions of the world, at least in some cases, may not be mirror-pictures of the world itself—a view similar to that of a famous physiologist in later times, who argued that we are not directly aware of the objects of sense but of the nerves that lie between the objects and our minds!

If John Locke, the Englishman, believed that we could know, directly or indirectly, about the physical world, his belief was certainly not shared by the equally brilliant Irishman whose views we may now consider. George Berkeley (1685-1753)—Dublin-born, a Trinity College graduate, a

bishop by appointment, and a philosopher by disposition—did not believe in the existence of material substance!

Belief in mind as the only true reality, although reflected to-day in some philosophies and in the doctrines of special sects, is obviously not characteristic of modern "common sense." The average person is more inclined to agree with Lord Byron that

> When Bishop Berkeley said "there was no matter,"
> And proved it,—'twas no matter what he said.

Nevertheless, the view, philosophically, is not at all unusual and is not as easy to disprove as its apparent absurdity would indicate. Moreover, in one form or another, it has even been taken seriously by some psychologists while searching for a definition of their science and seeking to determine its place among other sciences.

Since John Locke's secondary qualities (the colors, sounds, tastes, and so forth) showed his doubt of the existence of certain things in the outer world—at least as they were pictured in the mind—Bishop Berkeley can be said to have reached his position by going Locke one better. Berkeley denies that the mind pictures objects at all. A little thought may convince the reader of this disturbing possibility. Consider, for a moment, that the page now being read *might* not be physically "out there" at all, but only "in mind"! What is there, with absolute sureness, but so much sensation of a visual, auditory, or touch quality—certain modes of experience, so to speak—distinguished and named only through their "mental" differences?

One other, more concrete and understandable contribution of Berkeley is always mentioned by historians. It concerns the question how we know the distance from us to objects belonging to the world of sight. More specifically, how do we know the distance from us to this book, the picture on the wall, or the tree outside our window?

In spite of the fact that the reader may never have considered that there is a problem involved in his judgment of the distance or the solidity of seen objects—the so-called third-dimensional aspect of visual experience—it has long been a very annoying psychological problem. Leonardo da Vinci, the artist-scientist, recognized it, and so did Descartes; but it was Berkeley whose analysis was most complete and, for many years, conclusive.

Berkeley argued that we never sense visual depth, or the third dimension, directly, but always by means of *cues* or "criteria" whose meaning for such judgments we have learned to interpret; for how, he might have said, could an object-image, impressed upon the sensitive surface of our eye, tell us how far it has come—of the distance traveled before it reached that surface—any more than a letter that reaches us with postmark blurred or missing?

In his *New Theory of Vision* (1709) Berkeley describes the probable nature of these important cues or "stamps." First of all there is the matter of relative size. A quarter of a mile away we see the figure of a friend. His image impressed upon our eyes is, we may agree, quite small. Do we, therefore, judge that we have a shrunken friend? Not at all! We see nothing unusual in his size; we see him merely *at a distance.* And what holds for our friend holds for other objects too—the larger the nearer, the smaller the farther away—so that we may say, with Berkeley, that the relative size of objects is a criterion of their distance.

Besides this there are other factors. For instance, the object is judged nearer if it partially hides another from our view—the factor of "interposition"; while faint, unsaturated colors (e.g., the bluish haze of far-off mountains) are often associated with distance—the factor of "aerial perspective." Relative size, interposition, aerial perspective—all these cues were mentioned by Berkeley (in less modern

terms) as aids to our judgment of distance, and all of them have been the technical property of every painter for ages.

The next criterion is, however, not so obvious. Berkeley called it an "appreciation of the distance between the pupils of the eyes"; we speak of it as "binocular convergence." When a near-by object is focused, or "fixated," the two eyes converge (in extreme cases appear crossed), and an appreciation of this convergence, in terms of sensation from the eye-muscles, tells us of the distance of the object fixated —thus, the greater the convergence, the nearer the object, and vice versa.

The final criterion is that which we now call "accommodation" and explain by the change in the shape of the eyes' lenses in response to the contraction of tiny muscles attached to each. Objects very close to the eye require great contraction of these muscles; those that are three or four feet away require very little. Although unfamiliar with these details, Berkeley did recognize the influence and treated the sensations arising from such changes as another source of information relative to the distance of the looked-at object.

This highly specialized contribution is perhaps the closest to present-day psychological studies of any that I have cited. Had Berkeley made some attempt to verify his theory by appeal to a more objective and controlled experimental technique, by measurement of the conditions under which his criteria were operative in a group of persons, we might to-day call him the father of experimental psychology. But had he done so it would have been an exception to the usual slow course of historical development, and we cannot demand too much of one man—especially one whose interests were philosophic rather than scientific. The surprising thing is that George Berkeley, a "subjective idealist" among philosophers, should have come as close as he did to the solution of a scientific problem that still challenges us.

Just as the inclusion of one guest at a party often demands

the inclusion of others, so the temptation is very great to add many names to our list of the men who have been responsible in some sense for the nature of our present definitions of psychology. Perhaps, then, I shall be forgiven if I devote a sentence or two to each of a few more men, principally philosophers, who helped to set the psychological stage.

David Hume (1711-1776), a Scottish philosopher, historian, and statesman, did for Berkeley what Berkeley had done for Locke. To quote from a recent account of Hume's contributions to psychology:

> Locke had eliminated from experience all but the sense impressions and their combinations. He still accepted the existence of objects that were similar to our ideas. Berkeley went a step farther by his denial of the existence of objects at all. He found a justification for the ideas in the fact that God gave and guaranteed them. . . . Hume took the next obvious step by questioning the existence of God and the soul. This left nothing real except sensations and ideas.*

In addition to this, Hume made a clean-cut distinction, still prevalent, between these sensations (Hume said "impressions") and ideas (we say "images"); and he treated what we regard as "cause and effect" in our everyday world as a mere sequence of mental events occurring with such regularity and in such order as to give us the illusion that there is a necessary connection between two somethings in the objective world. The significance of these ideas will be apparent when we come to review some very recent opinions concerning the true business of the psychologist.

David Hartley (1705-1757), an English physician and scholar of Hume's generation, is credited with the development of two concepts, both of which he treated in a book bearing the homely title: *Observations on Man, His Frame,*

*W. B. Pillsbury, *The History of Psychology*, pp. 92-93.

His Duty, and His Expectations. The first was that of "association" (already considered by Locke, Berkeley, and Hume), which Hartley extended to include not only ideas but sensations and actions as well, and used to explain the nature of memory, imagination, emotion, and other complex mental states—even those pertaining to morals. The second concept was that which we now call "psychophysical parallelism," according to which sensations, ideas, and other mental events run alongside of, but are not affected by, events of a more bodily nature—specifically, physical changes in the nerves and the brain. (An earlier expression of this view had compared the mind and body to a pair of clocks, placed back to back, running in perfect time with each other but exercising no mutual influence.) Hartley was, like Descartes, a dualist, but a parallelist rather than an interactionist. Of the two views, contrary to common-sense expectation, Hartley's has been the more acceptable to the majority of modern psychologists.

James Mill (1773-1836), the son of a Scotch cobbler, was the intellectual descendant of Hartley. He made extreme use of the "association of ideas" in explaining mental life. Beginning in the usual fashion, with sensations and their copies, ideas, he pointed out in great detail how the latter may logically be connected and compounded. According to his teaching, as summarized by a well-known historian,

> Every experience is resolved into sensations and ideas, combined into groups, or following in trains, by the single process of association; and the principle of association itself is reduced to its simplest terms—the tendency of ideas to group themselves or succeed one another after the manner of their originals.*

John Stuart Mill (1806-1873) agreed with his father, James Mill, in attaching great importance to the principle of

* H. C. Warren, *A History of the Association Psychology* (Charles Scribner's Sons, 1921), p. 94.

association in explaining complex ideas, but, unlike his father, appealed more to experience than to logic in his analysis. To James Mill a complex idea—for example, the idea of "house"—was actually supposed to *consist* of many simpler ideas (ideas of "floor," "walls," "windows," and so forth), even though such ideas might escape the most careful scrutiny. John Stuart Mill argued that the simpler ideas *generate* more complex ones which are definitely more than the mere summation of the simple components. Because of this difference in theory between father and son, the former's view has often been called "mental mechanics" and the latter's "mental chemistry." Each supposed that the fundamental units of mind were sensations (as well as their copies, ideas) and these were united under certain prescribed laws of association; but John Stuart Mill was less interested in what *ought* to be found in a complex idea than in what *could* be found. Where he differed from his father he came closer to modern teaching.

By this time, perhaps, a certain trend of development in this sketch is apparent to the reader. We have seen how the mind came to be separated from the body; we have heard the argument that all ideas come from experience; we have made the distinction between sensations and ideas; and we have noted the elaboration of the concept of "association" to explain the formation of complex ideas and chains of ideas. In addition, we have had a glimpse of a psychological theory of "causality" (Hume); we have been given two or three different answers to the "mind-body" question—interactionism (Descartes), idealism (Berkeley), and parallelism (Hartley); and we have found one or two specific anticipations of present-day research and theorizing.

Yet it would be unfair to leave England behind without some reference to another theoretical concept that was destined to figure very prominently in the origin of American psychology. This is the famous doctrine of evolution. Two

names deserve special mention in connection with evolution: Charles Darwin (1809-1882) and Herbert Spencer (1820-1903).

Descartes, we might say, took mind away from the animals; Charles Darwin gave it back to them—with interest. In the opinion of Descartes, man alone possessed a mind. To Darwin, man's mind differed from the animal's only in degree, not in kind, and the higher form was a direct evolutionary outgrowth from the lower. This is a somewhat neglected aspect of Darwin's teaching, but a very significant one for the psychologist. Whatever we decide is the present status of "mind" in psychology we shall be forced to consider its development as well as its nature; and it was Darwin, above others, who made us recognize this obligation. We shall see this particularly in the development of a popular movement in American psychology known as "functionalism"; but the influence has not been confined to a single school. Even a careless thumbing of almost any psychological text will bring to light some vestige of this Darwinian doctrine.

In the writings of Herbert Spencer, British philosopher and scientist, the evolutionary principle took a different slant. Spencer was an associationist in psychology who undertook to reconcile the notion of "something innate" with that of "everything from experience" and thus gave us the view of "evolutionary associationism." The gist of this view is that man's mind is what it is through *racial* as well as *individual* experience. Oft-repeated associations, passed down through many generations, beget "automatic psychical connections" that have all the earmarks of innate ideas but are really traceable to ancestral experience. This doctrine of the "inheritance of acquired associations" is not taken very seriously to-day but it finds its echo in the modern question of what is learned and what is unlearned, or given, in human conduct.

It should not be too hastily concluded that the problem of defining present-day psychology originated solely in the arm-chair speculation of British philosophers, statesmen, and scholars. On the continent, particularly in Germany, there was another major stream of influence, more scientific in its nature, that contributed its full share to the growth with which we are concerned in this chapter.

This stream, which gained great volume and momentum in the first half of the nineteenth century, was physiological in nature. It comprised studies of all the senses—sight, hearing, taste, touch, and smell, as well as the newly discovered muscle (kinesthetic) sense; studies of the activity of human and animal nerves; even studies of the function of different parts of the human brain. Incidentally, these studies of how the brain works had their beginnings in the over-bold assertions of the now-discredited phrenology, according to which certain mental "faculties," comparable to our modern "personality traits," were related directly to the development of particular brain-areas (and thus to various skull-protuberances or cranial "bumps").

In one respect this work was an elaboration of the ideas of such theorists as Descartes and Hartley, but it went far beyond their crude imaginings of the structure and the functions of the human organism, and it offered a sounder basis for the speculations of the future. It was analytical and experimental in character and it pointed to the importance of a variety of agents (stimuli) in the excitation of sense-organs and nerves; to the high speed of transmission of nervous impulses over well-defined nerve paths; and to the specific localization of certain simple activities—such as those involved when we use our senses and move our muscles—in certain portions of the brain.

Many great scientists, English and French as well as German, contributed to this widespread movement. If I confine myself to the consideration of but one representative, it is

only because the bounds of space and time forbid the discussion of the contributions of many and because the man selected represents sufficiently well that combination of philosophic belief and scientific discipline which was so characteristic of early psychology.

Gustav Theodor Fechner (1801-1887) is best known today as "the father of quantitative psychology," yet he never intended to be a psychologist! Born in a little village of southeastern Germany, the son of a German Lutheran preacher; reared in studious surroundings and trained in medicine; subsequently a mathematician and physicist of distinction (with a flair for writing satirical poetry)—this is the story of the first half of Fechner's life. It was not, indeed, until 1850, after a serious and prolonged "nervous breakdown," that he became actively interested in the philosophical question of the mind's relation to the body—a question which led him, by virtue of his natural-science training, to experimental psychology.

We have already seen a recognition of this mind-body problem in the work of Descartes, Locke, Berkeley, Hume, and Hartley, but it was Fechner who saw the possibility of attacking it with the experimental method. Whether he solved it thereby is very doubtful, but in ten years of patient investigation he founded the science of "psychophysics"—the quantitative study of the relation between the mental life (Fechner dealt with sensations) and certain aspects of the physical world (stimuli).

It is not essential that we consider Fechner's psychophysics in any detail here. He made it clear once and for all that experimental techniques and mathematical procedures could be applied to psychological problems. The methods of measurement that he developed are brought into use to-day, in slightly modified form, whenever we try to find out anything really definite concerning the sensitivity of the human, or even animal, organism to the countless and dis-

turbing changes of the outside world. How bright the star in order to be seen; how loud the sound to be heard; how heavy the touch to be felt? To answer these and a thousand other questions we turn to Fechner's psychophysical methods.

What has this to do with the problem of defining psychology? The answer is plain. The work of Fechner (and others) showed quite irrefutably that no matter what one's philosophical opinions might be with respect to the mind-body problem there was still the possibility of constructing an experimental psychology. Something specific about human activity (Fechner called it something *mental*) could be measured and related in an exact manner to something else (he called it *physical*). The enormous bulk of meaningful material gathered together was the result of no mere accident, nor was it the fruit of philosophical speculation. Few psychologists to-day are aware of Fechner's mind-body views but none can ignore his experimental findings.

And this has been the story ever since. Whatever the conclusions reached as to psychology's true subject-matter, there is always that formidable and ever-growing body of scientific fact to justify the attempt at a systematic treatment. The boundaries of the sciences are never really very sharply defined, and a new field of research is not to be scorned on the basis of its temporary lack of a universally satisfying definition. Should this statement seem obscure to the reader, let him be patient; the fog will raise shortly, when we deal directly with the business of "system-making."

Chapter II

THE FOUNDING OF MODERN PSYCHOLOGY

BETWEEN "fathers" and "founders" of sciences a distinction may be made. Compare, for a moment, a science to a garden. The "fathers" till the soil and sow the seed; the "founders" do the weeding, the watering, the transplanting, and the fencing—they care for the garden in its early growth. The seed may be dropped by countless hands, by many of them carelessly; hence there may be many fathers, each unknowing of the part he plays. But the founders must be aware that a garden is in the making, and theirs is the arduous task of nursing and tending until helpers arrive. Founders are few.

I have called Aristotle the father of psychology; Descartes the father of modern psychology; and Fechner the father of quantitative (or experimental) psychology. Other candidates for such distinctions might have been proposed. Moreover, as specialization appears in the psychological garden, ancestry is easier to identify, and more and more fathers may be named, with more justification.

Fechner may, with good reason, be called the founder of psychophysics and the father of experimental psychology; he developed the former and he showed the way to the latter. It remained, however, for another distinguished German to become the true founder of modern experimental psychology; and in the treatment of this man's achievement we approach more closely to the solution of our problem of definition.

Wilhelm Wundt (1832-1920) was, like Fechner, the son of a German Lutheran pastor of a village parish, and also

Chapter II

THE FOUNDING OF MODERN PSYCHOLOGY

BETWEEN "fathers" and "founders" of sciences a distinction may be made. Compare, for a moment, a science to a garden. The "fathers" till the soil and sow the seed; the "founders" do the weeding, the watering, the transplanting, and the fencing—they care for the garden in its early growth. The seed may be dropped by countless hands, by many of them carelessly; hence there may be many fathers, each unknowing of the part he plays. But the founders must be aware that a garden is in the making, and theirs is the arduous task of nursing and tending until helpers arrive. Founders are few.

I have called Aristotle the father of psychology; Descartes the father of modern psychology; and Fechner the father of quantitative (or experimental) psychology. Other candidates for such distinctions might have been proposed. Moreover, as specialization appears in the psychological garden, ancestry is easier to identify, and more and more fathers may be named, with more justification.

Fechner may, with good reason, be called the founder of psychophysics and the father of experimental psychology; he developed the former and he showed the way to the latter. It remained, however, for another distinguished German to become the true founder of modern experimental psychology; and in the treatment of this man's achievement we approach more closely to the solution of our problem of definition.

Wilhelm Wundt (1832-1920) was, like Fechner, the son of a German Lutheran pastor of a village parish, and also

like Fechner, he was trained (at the University of Heidelberg) to be a physician. Like Fechner, too, his interests turned from the practical to the academic during the years of his formal education. Physiology, philosophy, logic, and ethics—all these fields claimed his attention at one time or another, but he was fundamentally a psychologist and, unlike Fechner, he knew it.

The time was ripe for the founding of modern psychology. Besides the intellectual legacy I have already discussed, there were many contributions from other fields. Physiology donated most. Early experimental psychology was based upon physiological techniques and findings. But in addition to this and the philosophical tradition there were problems bequeathed by astronomy, anthropology, and the study of hypnotism. It remained only for a man of Wundt's caliber to weave them all into the pattern of a new psychology.

In 1873-1874, after at least fifteen years of preparation, Wundt published his *Outlines of Physiological Psychology*, called by one historian "the most important book in the history of modern psychology"; in 1879, at the University of Leipzig, he established the first psychological laboratory in the world; in 1881 he inaugurated a scientific journal for the publication of psychological research. His book went into six revised editions and grew in size from one large volume to three; the laboratory prospered, and research students, drawn from near and far, filled the journal with reports of psychological experiments.

Wundt, himself, was tireless. Besides the work of instructing, administering, editing, and directing research, he wrote voluminously. His *Physiological Psychology* will be considered in a moment, but there were also books on other phases of psychology, as well as texts in philosophy, ethics, and logic to occupy his time. It has been estimated that he published, on the average, more than two pages a day for

sixty-eight years—and none of this material is "light" reading!

In the *Physiological Psychology* Wundt gives us our first "systematic" psychology; he tells us what psychology is; he outlines its methods of investigation; he points to its problems; and he classifies the results already obtained. Out of the breadth and the depth of his philosophic and scientific training, supplemented by the work of his laboratory, he brings us the first real handbook of the new science—and sets the standard for many future ones.

A little later I shall deal in some detail with the system of psychology set forth by one of Wundt's most illustrious pupils. This system was so similar to Wundt's—and so much easier to report—that we need not dwell to any great extent here upon the Founder's pronouncements. It will be enough to mention certain outstanding characteristics with which he stamped the new psychology.

First of all, Wundt, like many others since the time of Hartley, was a psychophysical parallelist in his attitude toward the mind-body problem. On the one hand there was the physical world, the world of material objects; on the other there was the mental, the world of experience. Psychology was to deal primarily with the latter, and it could, therefore, be defined as "the science of immediate experience." By "experience" Wundt meant to include all such phenomena as sensations, perceptions, feelings, emotions, and the like.

The method to be used by the psychologist was called by Wundt "introspection"—a term much misused in later days—which involved little more than the having of experience. The "having" was to be considered equivalent to the "observing" of consciousness. What I have previously referred to as "the world out there" was to Wundt merely so much experience or "mental process"; and when one *had* it, he had *observed* it.

The problem for psychology was really the problem of what to do about this experience scientifically, and Wundt's answer was threefold: experience was to be *analyzed* into its elements; the elements were in turn to be examined with respect to *the nature of their connections*, one with the other; and, finally, *the laws of their connections* were to be determined.

It should be apparent to the reader that these notions, especially of analysis and association ("connections") are by no means new in the history of our problem. Nevertheless, Wundt brought to them the ordered mind of a man trained in scientific modes of thought and accustomed to careful, rigidly maintained distinctions—in a word, the technique of the physiologist. There is a wide gap between the experienced "idea" of the British empiricist and the sensory or imaginal "process" of Wundtian introspection. For example, the idea of "elephant" or "everything" is a different kind of mental element than the sensation or image of "red" or "C-sharp," and only the latter would have been acceptable to Wundt as true elements. Analysis of "elephant" (into sensation, image, or both) might be possible—was, indeed, inevitable—with a careful description of experience, but a mental dissection of such a simple unit as "red" could not be made. Wundt proposed to get down to fundamental, irreducible elements before he undertook to show their relation to each other in the fusions and the combinations of everyday mental life.

Monumental studies in the physiology of vision, hearing, and the other senses had already been carried out by such men as Fechner, Weber (who anticipated some of Fechner's work), and Helmholtz (who is perhaps best known as a physicist). These Germans had provided much in the way of experimental analysis, so it is not strange that Wundt's text contained many of their results. In addition, however, the *Physiological Psychology* presented material on images,

feelings, action, attention, and a wealth of other processes. In fact there was practically nothing psychological that escaped the eye of the Founder; and it is no wonder that his textbook set the style for many years to come.

Finally, we return to Wundt's psychophysical parallelism. He believed that for every mental process there was a corresponding, concurrent physical process. Stimuli of the external world, acting upon sense-organs, aroused nervous impulses which, in turn, gave rise to brain activity. With the brain activity came mental activity, but the former did not truly "cause" the latter, nor could the latter cause the former. There were two distinct spheres of activity, one physiological, the other psychological; and "physiological psychology" seemed to Wundt the best way in which to designate the two-fold interest of the new psychology and the intimate relationship between the two fields of research.

We can now begin to see the shape and complexion of nineteenth century psychology. It was primarily a product of the union of philosophy and physiology. Its subject-matter was mind (experience, consciousness), its method was introspective, analytical, and experimental; and its problem was to describe the content or structure of mind in terms of elements and their combinations. In addition it dealt with questions of mental development and evolution, of cause and effect, of the innate and the acquired; and it had something to say about language, memory, thought, volition, and kindred psychological topics. Its philosophy was predominantly parallelistic, and it sought to explain the mind's relation to the body by the use of the methods of science.

Wundt's influence was tremendous. His pupils and his books carried his teachings to far-off parts of the civilized world, arousing a keen interest in the teasing apart of the mind with the "brass instruments" of physiology. New laboratories were set up in various universities, new courses

of instruction were offered, new psychological journals appeared, and new textbooks were written.

Ultimately, of course, new systems of psychology came into being. Our present task would be much simpler if they had not, but it is of the nature of any healthy science to grow and change, to revise its program from time to time. Differences of opinion were bound to arise, even among Wundt's most loyal pupils, as to the subject-matter, the methods, and the problems of psychology.

A system of psychology is, in a sense, nothing more than a logical framework into which may be fitted the findings of the science. It represents an attempt, usually by one man, to arrange and coordinate the facts of psychology in a simple, understandable fashion. When one man's system or point of view is found acceptable to a number of others, who take an active part in spreading its influence, a "school" of psychology is ordinarily the result. Not all systems beget schools, but a school cannot live without a distinct allegiance to a system. When that allegiance is lost the school disintegrates and must be remodeled or supplanted.

Wundt's system was not so much replaced as it was revised. It was propped up rather than broken down, and by one of the Founder's most distinguished pupils. It was not attacked by an outsider with no "school spirit" to dull the edge of his knife. Indeed, the Wundtian influence might never have been so great without the labors of the man who presented his own revised version of it to the English-speaking world.

Chapter III

TITCHENER AND STRUCTURALISM

EDWARD BRADFORD TITCHENER (1867-1927) was an Englishman by birth, a German by temperament, and an American by residence. He came to Leipzig in 1890, after a brilliant student career at Oxford, to learn at first hand about the new psychology. He had already translated into English the third edition of the *Physiological Psychology*. In two years' time he took his doctorate and accepted a call to America, to take charge of Cornell University's new laboratory of experimental psychology. There he remained for the rest of his life, thirty-five years, without becoming naturalized—in either a civic or academic sense.

At Cornell, Titchener did full credit to his master. He carried on the Wundtian tradition in the Wundtian manner —teaching, writing, and directing research—and with extraordinary ability. His scholarship was profound; his lectures and his writings were models of clear and dignified exposition; his personality was magnetic and forceful. Undergraduates flocked to his classes and graduates came to his laboratory. Cornell was soon the headquarters and clearing-house for a very important branch of experimental psychology in America. Titchener's may not have been the only psychology on this side of the Atlantic, but for two or three decades it was the best organized, most articulate, and the closest to the pattern set by Wundt. In our search for a definition of psychology we may profitably examine this

Leipzig-Cornell product in some detail, to see what Titchener thought psychology was.

Titchener's view changed somewhat from year to year, but we can get a very good notion of his principal systematic ideas from two of his published texts: *A Textbook of Psychology* (1910) and *A Beginner's Psychology* (1915). In these books, written primarily for sophomores and freshmen respectively, we shall find a more outspoken account than in some of his more advanced delineations prepared for his colleagues.

"Psychology is the science of the mind." This is the general statement with which Titchener begins his systematic account. But he hastens to add that this statement is easily misinterpreted by "common sense," and he goes on to qualify it in certain ways. "The mind with which psychology deals must be a mind that is describable in terms of observed fact"; it must not be identified with some insubstantial little being inside of our heads. In order to come nearer to a truly scientific understanding of the term, Titchener then makes a distinction between the world of physics and the world of psychology. Let us read again from the *Beginner's* text.

> The world of physics is colourless, toneless, neither cold nor warm; its spaces are always of the same extent, its times are always of the same duration, its mass is invariable; it would be just what it is now if mankind were swept from the face of the earth. For what is light in the textbooks of physics?—a train of electro-magnetic waves; and sound is a vibratory motion of air and water; and heat is a dance of molecules; and all these things are independent of man.
>
> Physics views the world with man "left out," so to speak; psychology, on the other hand, describes the world as it is in man's experience—it views the world with man "left in."
>
> The world of psychology contains looks and tones and feels; it is the world of dark and light, of noise and silence,

of rough and smooth; its space is sometimes large and sometimes small, as everyone knows who in adult life has gone back to his childhood's home; its time is sometimes short and sometimes long; it has no invariables. It contains also the thoughts, emotions, memories, imaginations, volitions that you naturally ascribe to mind . . . mind is simply the *inclusive name of all these phenomena.*

From these quotations it should not be concluded that there is any fundamental difference between the *experience* of the physicist and that of the psychologist. Titchener was not denying that the physicist had experience; he was merely emphasizing the well-known fact that the physicist's *description* of his world was in terms of conceptual things like electromagnetic waves, vibratory motions, and molecules. He might logically have gone further and made it clearer that the psychologist also describes *his* world in conceptual terms; but this is a subtle matter and we need not stop to quibble here.

Titchener tells us next that in the *physical* world there are such objects as human bodies, with nervous systems that organize them into single, integrated, organic wholes. We have learned from a variety of sources that the "phenomena" of psychology are to be related to certain activities of these nervous systems. For example, destruction of some portion of the brain is frequently tied up with the loss of some form of experience, say visual. Likewise, disturbance in experience, or lack of experience, may denote the loss of certain brain function. The "man left in" is, then, little more than the nervous system itself. Psychology might even be defined as the study of mind (experience, phenomena) *considered as dependent upon the nervous system;* for wherever we find experience or mental phenomena we also find a nervous system. Not all nervous events are paralleled by mental ones, but everything mental has its counterpart in something

physical happening in the brain as a result of stimulation of sense-organs and nerves.

The reader need not despair if this reasoning seems somewhat complicated. Much of it will be cleared up as we go into the less abstract details of Titchener's psychology. It may merely be noted in passing that there is implied in all this a philosophical dualism, a distinction between body and mind, that harks back to Wundt and beyond—even to Descartes, although Titchener would not have subscribed to interactionism.

The *method* of psychology is our next consideraton. Mind, to be studied scientifically, must be observed. Observation is the *sine qua non* of all science. Titchener felt, with Wundt, that *having* experience was very close to observing it; and he stressed the method of introspection. But his formula for introspective observation was more comprehensive than Wundt's, and it gives us a basis for distinguishing psychological from physical observation. He points out that *all* scientific observation requires three things: a certain *attitude* towards one's experience, the *experiencing* itself, and an adequate *report* of the experience in words. Where the attitude is that of the psychologist we call the totality of the observational process "introspection"; where it is the attitude of the physicist that is involved we call the process "inspection"—or just plain "observation." Difference in attitude alone sets off the observations of the psychologist from those of any other scientist.

"Introspection" is an unfortunate word in that, because of its history and its everyday usage, it is open to misconstruction. Titchener realized this and took pains to show that it was *not* to be thought of as a reflection upon, or contemplation of, one's experience (as Descartes and the British empiricists would have considered it) or as a morbid sort of self-interest (for which a better term is "introversion"). In fact, Titchener was ever on the alert to show that scientific

terms in general should always be used carefully and unequivocally, and he frequently pointed to the confusion that arose when "common-sense" meanings were given to scientific words.

When we read Titchener's statement of the *problem* of psychology, we see clearly the imprint of his Leipzig teachings. The problem is again threefold. There is, first, the *analysis of mental phenomena into their elements.* A description of any cross-section of experience is bound to be an analysis, for we analyze whenever we describe—we break down the object of our observation into certain fundamental parts. (If the reader wishes to test the truth of this, let him try to describe any common object near at hand. Analyzing is one of the most natural human activities in the world, except that it is seldom carried so far as to become very scientific.)

Synthesis, although more difficult than analysis, goes hand in hand with it. It involves the study of the connections between the elementary mental processes and is the road to the determination of *laws of connection* of these processes. This is the second phase of our problem, and it answers the question "How?" just as analysis answers the question "What?"

The third aspect of the problem goes beyond the description of mind (and beyond Wundt's statement of the problem of psychology) to the *explanation* of mind. It aims to answer the question "Why?" and, in so-doing, it appeals to the parallel events in the nervous system and its attached organs; it endeavors to *correlate mind with the nervous system.* Titchener denied that nervous activity was the *cause* of experience; but he affirmed that a thoroughgoing statement of the *conditions* or circumstances under which mental processes occurred demanded a reference to this activity, for explanation's sake. "Dew is formed under the condition of a difference of temperature between the air and the ground;

ideas are formed under the condition of certain processes in the nervous system."

So much for the statement of the fundamental premises of the Titchenerian system. We are now entitled to examine the results of such an attack upon the fortress of mind, to see how the system handled the experimental research that was the real basis of the new psychology.

Introspective analysis, said Titchener, brings to light three main classes of mental elements. (This number decreased with the development of Titchener's ideas; we take them as of 1910.) These classes are *sensations, images, and affections* (*feelings*). They may be dealt with in this order.

"Sensations are . . . the characteristic elements of perceptions, of the sights, sounds and similar experiences due to our present surroundings." They may be divided into various "modalities" or departments according to (1) their introspective similarities (e.g., a tone is more like a noise than it is like a taste); (2) the sense-organs upon whose functions they are conditioned (e.g., there is eye-sensation and ear-sensation); or (3) the types of stimuli which condition them, located either within or without the organism. Titchener depends mainly upon their introspective differences in classifying the departments, but uses the other methods when satisfactory group-names are lacking.

The principal modalities of sensory elements are then listed. We find seven in all: the visual, auditory, olfactory, gustatory, cutaneous, kinesthetic, and organic senses. Each of these, in turn, may undergo further analysis and subdivision. Thus, vision yields color and light—chromatic and achromatic—sensations; audition yields tones and noises; and smell gives us a variety of sensations that may be placed in somewhat distinct groups (such as fragrant, spicy, and foul odors) on the basis of similarity and difference. Cutaneous sensations are divisible into sensations of pressure (true touch), cold, warmth, and pain; and kinesthesis, the old

"muscle" sense, shows itself to contain muscle, tendon, and joint components. "Organic" turns out to be a general term for all those ill-defined sensations arising from the digestive, urinary, circulatory, respiratory, and genital systems.

A word of qualification may be appropriate at this point. Titchener did not *discover* these sensations. Neither did he discover the laws of their relation to environmental stimuli, with which he deals extensively in his texts. This work of discovery, classification, and correlation had been started ages before, even in Aristotle's day, and had reached a high degree of exactitude in the studies of the nineteenth century physiologists whose work has already been mentioned. What Titchener did was just what other systematists had done, and still do: he appropriated these facts (adding items from his own laboratory) and arranged them within his system—for their better integration and illumination.

From Titchener's point of view, sensations were mental elements comparable to the elements of chemistry. One defines chemical elements by reference to certain properties such as their power of reflecting light, their specific gravity, their fusibility, etc. In the same manner one may characterize mental elements by reference to certain properties that they possess or do not possess. Thus we arrive at Titchener's notion of *attributes*.

Sensations, as irreducible units of the mental world, possess certain characteristics to which we refer when we want to describe these units in minute detail. For example, all sensations, of whatever sort and from whatever source, possess the attribute of *quality*. This is the attribute by virtue of which we *name* sensations. B-flat, pink, warm, and sour are such qualities, and they serve to distinguish one sensation from another.

A second attribute of all sensations is *intensity*. This is recognized whenever we consider the strength or degree of a sensation. A tone may be loud or faint, a pressure light

or heavy, an odor weak or strong; we are aided in our description by these intensive characteristics. Incidentally, it was this attribute of intensity with which Fechner dealt most in his psychophysical studies, for it lends itself readily to quantitative statement. Theoretically, although not practically, any sensation might even be given a numerical designation on a graded scale from least to most intense.

Quality and intensity are the most important attributes of sensation, but there are others. Titchener lists these, in 1915, as duration, vividness, and extension.* *Duration* refers to the time-aspect of sensations: "it is the bare going on, going forward, keeping like itself, that may be observed in any and every sensation." *Vividness* is difficult to describe: "if you want to know how . . . vividness feels . . . observe your mental processes now, as you are puzzling over this book; the difference between foreground and background, focus and margin—between the dominant ideas aroused by what you read, and the obscure perceptions derived from your surroundings,—will show itself at any rate in the rough." *Extension* is the elementary space-factor in experience, just as duration is the elementary time-factor; "it is the basis . . . of our perceptions of form, size, distance, locality, direction"; the tiniest star in the evening sky, or the raindrop on the outstretched hand—both have perceptible size.

The first four of these attributes—quality, intensity, duration, and vividness—are properties of all sensations; but only visual and cutaneous elements, such as colors and pressures, clearly possess an extensive attribute, the removal of which would nullify the sensation itself.

Furthermore, even such a simple attribute as quality may itself be a resultant of two or three distinct attributes, the detection of which is the very ultimate in introspective anal-

* Later they became respectively "protensity," "attensity," and "extensity."

ysis. The visual quality "red," for example, is a combination of such attributes as saturation, brilliance, and hue, all of which are qualitative. We need not, however, go into these more delicate matters here. There are other mental elements than sensations to be considered in this survey of Titchener's teachings.

David Hume, as we have seen, was one of the first to distinguish between sensations and images, calling them respectively "impressions" and "ideas," and thinking of the latter as "faint copies" of the former. But Hume was also shrewd and observing enough to see that it was often difficult to tell the two apart on the basis of experience alone:

> It is not impossible but in particular instances they may very nearly approach to each other. Thus in sleep, in a fever, in madness, or in any very violent emotions of the soul, our ideas may approach to our impressions: As on the other hand it sometimes happens, that our impressions are so faint and low, that we cannot distinguish them from our ideas.

Except for the minor difference of terminology this states almost exactly the position of Titchener, one hundred and seventy-six years later, with respect to the question of sensations and images as mental elements of equal dignity and rating. He goes a few steps beyond Hume, however, when he says, in the *Beginner's Psychology*, that "it is very doubtful if there is any real psychological difference between sensation and image"; but, like Hume, he falls back upon a non-psychological difference in defining the image as "an elementary mental process, akin to sensation and perhaps indistinguishable from it, which persists when the sensory stimulus is withdrawn or appears when the sensory stimulus is absent."

If this seems obscure to the reader, let him recall that the psychologist's function, according to Titchener, was to de-

scribe experience, and experience alone. Stimuli are *not* mental processes, although they may give rise to them and may be considered in relation to them. Thus, when he says that sensations and images are perhaps indistinguishable, he means only that on the basis of introspective observation *alone* we cannot tell the difference: nothing about the mental process itself says "I am sensation" or "I am image." (Whether Titchener took seriously his own suggestion, in another context, of an introspectively observable "textural" difference between the two, may remain an unanswered question.)

Titchener finds images for every sense, with the possible exception of kinesthesis, and he finds them of different kinds within a sense. Besides the visual, auditory, and other modality images, there are such types as *recurrent* images (e.g., the tune that runs in our heads), *hallucinatory* images (the doorbell's ring, when no one pushed it), *dream* images, *memory* images, and so on—the list is long. These images, like the sensations, have their attributes of quality, intensity, duration, and the like; and, with sensations, they go far towards supplying the elementary components of mental life.

There remains to be discussed Titchener's third class of elements—the *affections* or *simple feelings*. These are defined by contrast with the elementary process of sensation. An affection differs from a sensation in the number of attributes it possesses; it lacks clearness (vividness) and it lacks extension. It may vary in the length of time it occupies (duration); it may be of greater or less degree (intensity); and it always has one of two qualities—*pleasantness* and *unpleasantness*. Never do these two qualities exist at the same time (there are no "mixed" feelings); and never must this form of feeling be confused with the "feelings" of the popular vocabulary. When we say "this feels rough or smooth," "I feel good," or "he feels that I am right," we are trying to make the term fit experiences that are far more complicated

in their nature and in which true feeling (affection) plays at best an inconsiderable rôle. (Obviously, we should not confuse this "affection" with that of the lover-sweetheart or parent-child relationship, no matter how much pleasantness and unpleasantness either may involve!)

In his treatment of affection Titchener departs vigorously from Wundt's teachings. The latter had been unwilling to recognize the status of pleasantness and unpleasantness as qualities of elementary processes, and had given equal rank to such feelings as tension and relaxation, excitement and calm. Titchener examines Wundt's theory in detail, as well as the experimental evidence behind it, and comes out for the elementary character of affections alone, and the combination-character of Wundt's other categories of feeling. He argues that tension, relaxation, excitement, and calm are really "sense-feelings," combinations of organic sensations and true feelings.*

So much for mental analysis. As we turn from the elements of Titchener's system to the combinations of these elements we go from the simple to the complex. We deal with such mental structures as perceptions, ideas, and emotions; with association, memory, and thought; even with such complicated things as sentiments and the "self." Full justice cannot be rendered to all these topics here, but certain general principles may be outlined and illustrative material presented that should help the reader to get the "feel" of the Titchenerian system.

Perceptions and *ideas* come up for consideration first. They are matters of everyday experience that offer themselves for analysis. Only when we take the laboratory attitude do we appreciate their compound nature. They are

* The controversy between pupil and master need not detain us here. Besides, Titchener, before his death, had reached the conclusion that even the feelings of pleasantness and unpleasantness were probably reducible to sensations.

the units of our daily mental life just as sensations, images, and feelings are the units of psychological analysis. The perceptions may be analyzed, with careful introspection, into (1) a number of sensations which are supplemented by (2) various images and (3) "moulded by the action of nerve-forces which show themselves neither in sensation nor in image." It is to be noticed that only the first two of these characteristics are really "experiential"; the third is an inference, not a true mental content but something lying *behind* it. An example: the *core* of our experience of, say, a tree is no more than an arrangement of color sensations. With this sensation-core come certain supplementary images —it is the tree that shades our neighbor's flower-bed in summer, the tree that caused the lawsuit, the tree that wore "a nest of robins in her hair." Moreover, the tree is taken automatically to be a real "thing" occupying real "space"; and this characterization may be founded upon no mental content whatever—either sensation or image—but be due merely to a kind of brain-habit that lacks a representative in the assembly of elements.

Just as a perception is a compound affair in which sensation figures prominently, so a typical idea is a mental structure possessing a nucleus of *images*. "Last winter's snow may come to us . . . as a visual picture, an uneven spread of white, with streaks of grey-brown on the peaks and along the valleys, honeycombed and broken from some partial thaw." This is the imaginal core—the basic image-content of the idea. But there is more. Other images may cluster about this core: "we recall the day so-and-so got his feet wet, or the big fall of that December Thursday." Even this is not all. "We can hardly think of getting our feet wet . . . without some actual movement that arouses sensation." The idea may, then, include sensation material that adds to its complexity. Finally, as in perception, a "brain-habit" may lie behind our idea as a molding or determining factor.

Feelings (pleasantness and unpleasantness), combined at an elementary level with certain sensations, primarily kinesthetic and organic, give us "sense-feelings." There are six classes of these sense-feelings: the exciting and the calming, the straining and the relaxing, and the agreeable and the disagreeable, each dependent upon the peculiar nature of the sensation-feeling blend. Each may combine, in turn, with further sensory and imaginal processes under certain conditions to produce such *emotions* as joy and fear, anger and grief, hope and relief.

There is no need of going further with this aspect of psychological compounding. Its general nature and the direction it took should by now be clear; and there are two or three other matters that are worthy of a brief review before we conclude our inspection of the house that Titchener built.

The first of these concerns a question that was asked, and answered, as long ago as 1709, in Bishop Berkeley's *New Theory of Vision*, and a quotation from the good Bishop's writings will introduce us to the problem here.

> Sitting in my study I hear a coach drive along the street; I look through the casement and see it; I walk out and enter it. Thus, common speech would incline one to think I heard, saw, and touched the same thing, to wit, the coach. It is nevertheless certain the ideas intromitted by each sense are widely different and distinct from each other: but, having been observed constantly to go together, they are spoken of as one and the same thing.

Berkeley offers this merely as an example of the manner in which "mind" generates "matter" (more specifically, "things" or "objects") by the combination or association of certain ideas. We have already had an illustration of the same kind of reasoning in his theory of distance-perception. The student of history, however, may find in this quotation

an anticipation of a much better known theory—namely, Titchener's *"context theory of meaning."* *

An obvious characteristic of our perceptions and ideas, according to Titchener, is that they have meaning. Last winter's snow, the tree in the yard, the rumble of Bishop Berkeley's coach—these are meaningful events. But what, psychologically, i.e., introspectively, is meaning? The answer to this question may already have been given in my discussion of perception and idea, but it will do no harm to elaborate.

When we analyze mind introspectively we find, not meanings, but mental processes—sensations, images, feelings, and their combinations. The tree of our example we found to be so much sensation, *plus* an accretion of images; last winter's snow turned out to be so much image, *plus* more imagery and sensation. It is in this plus-factor of mental processes that we shall find the answer to our question.

"Meaning," says Titchener, ". . . is always context; one mental process is the meaning of another mental process if it is that other's context." Context itself is nothing more than "the fringe of related processes that gathers about the central group of sensations or images." In perception and in idea there is *core plus context,* and the latter "carries" the meaning of the former, i.e., it is what we find when we scrutinize experience in our search for the counterpart of everyday logical meaning.

Titchener offers a number of illustrations to show the wisdom of this core-context distinction. Context may, in

* It may help, in this discussion, to parallel Berkeley's example with another. I hear the sound of a bark, and I say "there's my dog"; in the distance I see a moving object of a familiar shape, I say "my dog"; I feel a cold wetness on my exposed ear as I lie in bed in the morning, "my dog"; I smell a pungent barnyard odor, and again I say "my dog." The sensory core of these perceptions is different in each case, coming as it does from a different sense-department; but each perception *means* "my dog." *Why* it does so is the question that now concerns us.

some cases, be stripped from the core—as when we repeat aloud some word until the context disappears and the word becomes meaningless; context may be added to core—as when we learn the meaning of some strange design or foreign word; context and core may be disjoined in time—as when we know what we want to say but need time to find expressive words, or when the point of a joke is delayed in appearance; the same core may have several contexts—as implied in our worry about the true meaning of a chance remark; the same context may accrue to different cores—as in the case of Bishop Berkeley's coach; and so on. Titchener did not lack for examples to support his distinction. His inability, however, to demonstrate that meaning was *always* context is seen in his admission that it might be carried by a "brain-set" in the absence of conscious representation—as when the skilled reader grasps the meaning of a printed page, or a musical compositon is rendered in the appropriate key, *without* the presence of a fringe of images to supplement the core of the perception.

Context accrues to core *associatively*. Titchener made no outright statement to this effect, but it is clear that we can understand such compounding in no other fashion. We may, then, briefly inspect Titchener's treatment of association as a psychological principle, and thereby see what happened to the old British doctrine in the hands of an experimentalist.

"Whenever a sensory or imaginal process occurs in consciousness, there are likely to appear with it (of course, in imaginal terms) all those sensory and imaginal processes which occurred together with it in any earlier conscious present." This statement, which is the fundamental "law" of association, is taken from the *Textbook* of 1910, where it is preceded by a painstaking criticism of the older associationists and followed by an elaborate set of qualifying and amplifying clauses which we need not consider here. It is

meant to be a *descriptive* formula for the observed facts of the older doctrine; Titchener's *explanatory* formula appeals to the neural events running parallel to the sensory and imaginal processes above-mentioned.

The Law of Association became very important in Titchener's system, particularly in his treatment of memory and imagination, but Titchener himself realized that it was not all-sufficient for the understanding of mental connections. This realization is apparent in the very wording of the law. Note that he says "are *likely* to appear" (my italics) when he speaks of the associated sensory and imaginal processes. Remember, too, his use of a "brain-habit" as a molding or determining factor in the construction of perceptions and ideas. This amounts to a recognition that mental processes, and actions as well, arise not only as a result of the strength of associative bonds, but also because of certain directive forces—"brain-habits," "instinctive tendencies," "nervous sets," and so on—that may even work against the influence of oft-repeated associations. Thus, in addition to associative tendencies, we have "determining tendencies." Sensory and imaginal processes which, on the basis of frequent past association, should be joined together in mind, may, because of the pressure of some determining tendency, be kept apart—be only "likely" to appear. A simple example: the word "black" might, by virtue of association, call up "white" in the reader's mind, and "bitter" might similarly elicit "sweet"; but what would have happened if he had been told ("set" or "determined") to find *rimes* for "black" and "bitter"?

Our present purpose does not demand a more comprehensive and complete portrayal of Titchener's point of view. I have set down here, in spite of what the specialist might tritely call "glaring omissions," a working outline of his system—enough, at least, to provide us with a scheme of reference when we examine other views. There is experience

(mental process); it is to be analyzed introspectively into elements (sensations, images, and feelings), with their attributes (quality, intensity, and so forth). The elements are fused or patterned in space and time (associated) to give us such mental structures as perceptions, ideas, sense-feelings, emotions, and the like. Finally, these processes—both the simple and the complex—are paralleled by events in the nervous system and, to some extent, determined by them.

This Leipzig-Cornell product—by Titchener out of Wundt—was for more than two decades the psychology against which other psychologies matched themselves. As a system or school it came to be known principally as "structural" or "introspective" psychology—for reasons that should be clear with a moment's reflection, and will be even clearer later on. The most recent label, advanced by Titchener himself, is "existential," a word chosen to emphasize the fact that the world of mental process ("bare existence") is the only world that science can know.

Chapter IV

AMERICA AND FUNCTIONALISM

THE shifting ground, the new discoveries, the changing values—even the fads and fancies—of any young science make it very unlikely that a given school of thought will last for long, to the exclusion of all others. Certainly it will not survive without invasion in some quarters and rebellion in others; and especially will this be true in a country where there is notably a fine disdain for precedent and a high regard for the practical. It was not to be expected that the Titchenerian dogma should go unchallenged in America.

The attack upon the orderly, disciplined ranks of structural psychology was typically early-American. It came from no single, united front, under command of a single recognized leader. Instead there was a guerilla warfare, with many chieftains, striking in from many points of vantage, with many weapons.

This rebel psychology may or may not have been a school, or even a system, in the strict sense of these words, but it was undeniably an important *movement* and we cannot ignore it in our search for psychology's definition. Its doctrines are presented in no single text by no one outstanding sponsor, but they played a great part in determining the form and content of present-day psychology. If, then, in what follows, this movement is referred to as a "school" or "system," no grave injustice will be done.

"Functional" psychology took its form in the hands of a number of Americans, in particular, a small group of anti-

structuralists at the University of Chicago, during the closing years of the last century and the opening years of this. John Dewey (1859-), the philosopher and educator, and James R. Angell (1869-), until recently president of Yale University, are the principal names, but there were a number of others whose interests and sympathies—or antagonisms—inclined them in the same direction. Among to-day's representatives of the movement, Harvey Carr (1873-), senior psychologist at Chicago, is perhaps best known. These men were seldom in complete agreement as to systematic doctrine, and they do not seem to have worried much about the matter, yet there were certain general characteristics that marked them as a group.

(1) They were opposed to structuralism, of either the Wundtian or Titchenerian sort. Theirs was the first active protest in this country against the kind of psychology that blossomed in the Leipzig laboratory and seemed in the way of becoming the *only* psychology in Germany and America. The reason for this protest will soon be apparent.

(2) They were interested in what the mind *is for*, rather than what it *is*—the *function* of mind rather than its *structure*. They sought to answer such questions as these: What do mental processes accomplish? What difference do mental processes make? How do mental processes work?

(3) They were a practical-minded group, not above joining hands on a common-sense basis of values. Titchener had little respect for common sense and tried valiantly to keep the skirts of the new psychology clear of the purely useful. The functionalists had no such compunctions; both Dewey and Angell ultimately deserted psychology for practical, i.e., educational, pursuits, and others of the group showed similar leanings.

(4) They represented the biological rather than the physiological tradition in psychology. They were influenced less by the careful laboratory studies of nineteenth century

physiology than they were by the stimulating concepts of Darwinian biology. Impressed by the ideas of mental evolution and "survival of the fittest," they sought to determine the place occupied by mind in helping the human or animal organism to hold its own in the struggle for existence.

(5) They demanded that the field of psychology be broadened to include the methods and findings of animal, child, abnormal, differential, and other types of psychological research—research that was attracting more and more the attention of serious-minded students in this country and abroad, but which had never quite achieved respectability in the classrooms of structuralism.

(6) Finally, these men were, at one time or another, directly or indirectly under the influence of a man who has properly been called the "dean" of American psychologists, a man who is a symbol of the very best in functionalism without being a real member of this or any other school.

William James (1842-1910) cannot be pigeon-holed for purposes such as ours. Physiologist, psychologist, philosopher, teacher, writer—these are pitifully inexpressive titles for this genius of the Western Hemisphere! The reader will have to go to James himself *—for instance, to his *Principles of Psychology*—if he wants to feel the force of his personality and appreciate the effect of his teachings upon the character and destiny of American psychology.

It will have to suffice, in this context, to note that James opposed the "brass-instrument" † psychology of Wundt, with its hair-splitting dissection of mental content; argued that mind was a "personal," "changing," "continuous," and "selective" agent, "dealing with objects other than itself";

*Or to Professor R. B Perry's excellent two-volume book, *The Thought and Character of William James* (Little, Brown, and Company, Boston, 1936).

†"Brass-instrument" is a facetious characterization of that kind of psychology which utilizes the apparatus and techniques of the physiologist or physicist in the investigation of mind.

and promoted the Darwinian view that it had evolved for the purpose of "steering a nervous system grown too complex to regulate itself." And he did all this, and more, with so much skill and vigor, with such neatly chosen testimony and so many lively illustrations, that no intellectually alert American—after 1890, when *The Principles* was published—could very well ignore him. He did not establish a school, or belong to one; his system was his own, and he never "promoted" it. His interests led him soon to other fields. He left, however, an indelible, though indescribable, mark upon the new psychology.

With these general characteristics of functionalism in mind, we may now consider the system itself. This will not be an easy task, and fault may be found with this account because it has been necessary to pick and choose from various sources to show the distinctive qualities of the system. Among the members of the functionalist group there was no great stress upon system as system, and no such unanimity of belief—I might almost say, horror of heresy—as prevailed in the camp of structuralism.

The *subject-matter* of psychology, to the functionalist, was "mental activity." This is a term employed by Carr, one of the most systematic of the group, who has done much to crystallize and modify the school's teachings for present-day consumption. Mental activity is a broad term, not to be confused with Titchener's "mental process." It refers to such processes as thinking, feeling, imagining, perceiving, and the like; *not* to such elementary processes as "red," "sweet," or "B-flat," or even the perceptual and imaginal complexes of which they are components.

Mental activities are definitely distinct, but very elastic, categories. Each individual mental activity may involve any number of mental contents such as the structuralist described, none of which need be exactly duplicated from observation to observation. Thus, a given activity such as

"thinking" might never be reinstated with the same content of ideas or images; but "thinking" as a *function* of the human organism might be repeated again and again. We might, for example, "think through" a certain mathematical problem many times without ever using the same mental images, yet the function involved would be the same with respect to the most important factor—the arrival at a correct solution.

Moreover, mental activity is described as *"psychophysical"* activity. This conception, sometimes difficult for even the advanced student to comprehend, is central to functionalistic doctrine. Dewey argued that the mental and physical aspects of experience could not be treated as distinct in psychology. In the words of a recent reviewer: "mental acts are not psychical events pure and simple; they are events in which both the physical and the psychical are present." * Carr writes to the same purpose when he says that mental activities "are not only experienced, but they are also reactions of a physical organism. . . . They are acts of which the individual has some knowledge . . . that directly involve such structures as the sense organs, muscles, and nerves." Neither of these eminent functionalists would admit the fruitfulness, for psychology, of any sharp split between the body and the mind.

The *methods* of psychology, according to such a functionalist as Angell, are *introspection,* or "subjective observation," and the *"objective observation"* of physical science. Carr says: "Objective observation refers to the apprehension of the mental operations of another individual in so far as these are reflected in his behavior. Subjective observation refers to the apprehension of one's own mental operations."

The advantages and disadvantages of each type of observation are discussed by Carr in his *Psychology* (1925). He points out that, whereas introspection provides a "more

* Edna Heidbreder, *Seven Psychologies* (The Century Co., 1933), p. 213.

intimate and comprehensive knowledge of mental events," it is a difficult form of observation for the average person. The kaleidoscopic changes in the mental life elude all but the trained observer, and even *his* report is unverifiable because of its essentially personal nature. The objective method should, wherever possible, supplement the subjective, and it is obviously preferable in studying animals, children, primitive peoples, and the insane.

The *problem* of psychology, for the functionalist, seems, at the first blush, to resemble that of structuralism; both seek answers to the queries "What?", "How?", and "Why?" But scrutiny reveals a significant difference in the meaning of these questions for the rival camps. We are already familiar with the threefold Titchenerian statement of the problem—"analysis into elements," "laws of connection," and "correlation of mind and nervous system." Functionalism presents the matter in an entirely different way.

Generally speaking, it may be said that the functionalist's task is to discover (1) *how* mental activity goes on, (2) *what* it accomplishes, and (3) *why* it takes place. All three phases of the problem are but aspects of a single, comprehensive relationship between organism and environment —a relationship that will become clearer when we consider two concepts that resulted from the functionalist's way of thinking about psychological matters: the "reflex arc" concept, and the concept of "adaptive behavior."

The "reflex arc" was anticipated by Descartes in his bell-wire–tube explanation of animal (and human) movement. Descartes recognized that many muscular reactions followed directly upon appropriate stimulation of sense-organs by various physical agencies; and he designed a nervous system that would account for the connection between muscles and sense-organs, between responses and stimuli. The physiologists of later times rediscovered this connection in their experiments upon animals, and the term

"reflex" came into vogue as a designation of the "unlearned," "unconscious," "involuntary" reactions of animals to certain environmental excitations. Such reactions were considered as due to the transmission of some influence from sense-organ to muscle (or gland) over a connected series of nerve fibers. This was the "reflex arc." The squirmings of a decapitated newt or snake when tapped upon the skin, the slavering of a hungry dog at the sight of food, the narrowing of the pupil of a cat's eye when exposed to bright sunlight—these were typical observations upon which the reflex-arc concept was founded.

The work of these physiologists did much toward promoting the view that human and animal behavior was, at least in part, susceptible of analysis into reflex units or elements—just as structuralism promoted the idea of mental elements. Those psychologists who were interested in the *behavior* of organisms, or in the physiological correlates of mind, were quick to seize upon the reflex arc as an explanatory principle. It looked for awhile as if all or nearly all of the activities of animals, and even humans, might be describable, at least in their physical aspects, as chains or patterns of a large number of elementary reflex arcs.

Functional psychologists were interested in conduct as well as consciousness—in physical as well as psychical activity. Moreover, they believed that many truly psychological events had no appreciable mental aspect. It came very natural to them to accept the reflex-arc concept, and they did so, but only with such reservations as were necessary to make it compatible with, and illustrate, their general point of view.

According to Carr, three principles may be deduced from the reflex-arc concept. The first is that "all sensory stimuli exert some effect upon the activity of the organism"; the second is that "all activity . . . is initiated by sensory

stimuli"; and the third is that "there is a continuous process of interaction between sensory stimuli and motor response."

In connection with the first of these principles Carr reminds the reader that, in the environment of any individual, there are countless physical changes of greater or less degree. When some of these changes are adequate to bring about changes in the individual's sense-organs (organs of taste, smell, hearing, and so on), they may properly be called "sensory stimuli." These stimuli, acting upon the sense-organs, always initiate activity that is passed along over the nerve-paths to some muscle or gland, eliciting some sort of response in these structures. In certain cases the response is very noticeable, as when a person jumps from his bed at the sound of a bell or a motorist stops at the shift of a traffic light. In other cases the response may be so slight as to be detectible only by the most delicate of recording devices, as when the effect of a whispered threat or the tiniest prick of a pin can only be perceived as a change in the electrical resistance of a person's skin or in the quickening of his pulse. In *all* cases, however, according to this "principle of organic behavior," sensory stimuli evoke *some* sort of response.

The second principle is the corollary of the first, but is, perhaps, less acceptable to the reader's common-sense point of view. It holds, in effect, that there is *no* response without a stimulus, that muscular or glandular reactions may not be aroused by nerve impulses in the absence of some stimulus that releases the energy stored within the organism's "sensory-neuro-muscular" mechanism. This is an assumption that is not easily demonstrated. The initiating stimuli for many responses are very difficult, if not impossible, to discover. Carr points out that responses may be due to stimulation from *within* the organism—stimulation that acts upon the sense-organs lying in the muscles and other internal bodily tissues. "For example, hunger, thirst, and internal

pains are very powerful stimuli that largely determine the nature of the organism's reactions."

The third principle emphasizes the interplay between stimulus and response. "Every movement resulting from a sensory situation inevitably modifies that situation, and this change or modification of the sensory situation constitutes a new sensory stimulus which in turn modifies the act that produced it." This is an important component of the reflex-arc concept, for it stresses the fact that the nature of sensory stimuli depends upon the responses just as much as the nature of the responses depends upon the stimuli—a fact that must be considered in dealing with any sample of stimulus-response activity. An example: in the midst of writing this page I hear the ring of the doorbell (a change in my sensory situation); I get up from my chair and open the door of my room (responses to the change); from the living-room below I hear the murmur of voices (my responses have provided new sensory stimuli); listening, I recognize the voice of a long-absent friend (my "listening" response changes the situation once more); and I hurry down the stairs (response to the new change). In this example there are many neglected stages, but it illustrates Carr's point—that every response alters the sensory situation and thus determines in part the nature of subsequent responses.

For purposes of scientific analysis we are justified in dissecting the conduct of an organism into such reflex units as those of the above example, but it is an everyday matter of observation that much of an organism's behavior possesses a character that my illustration was not intended to bring into prominence—namely, the character of being "adaptive"; and this brings us to a consideration of our second functionalistic concept.

"Adaptive behavior," according to Carr, involves "a motivating stimulus, a sensory situation, and a response that

alters that situation in a way that satisfies the motivating conditions." This is the concept in a nutshell; and, to the reader who has fully grasped Carr's notion of the reflex arc, it should be apparent that the two concepts bear a strong resemblance to each other.

The new factor to be considered is the "motivating" stimulus, or condition, with which the second concept deals. A *motive,* to Carr, is always a stimulus—generally an internal one—and is practically identical with what some psychologists call "organic need," "urge," or "drive." He defines it as "a relatively persistent stimulus that dominates the behavior of an individual until he reacts in such a manner that he is no longer affected by it." What we call hunger, thirst, and the like would be typical examples—even the pain of a splinter in one's finger or the temperature of an overheated room. In fact, almost any stimulus might be a motive by virtue of its insistence or dominance, or because of the organism's readiness to react to it. The steady drip of water upon a shaven head has been known to constitute a very effective motivating stimulus; and the faintest cry of an infant may wake a mother from her sleep.

In addition to the motivating stimulus or condition, there is a "sensory situation," a wider environment, to which the individual may react even while responding primarily to the motive itself. One might even say that an organism *reacts* to a situation as a whole while *adapting* to but one aspect of it. The responses of a hungry man at the dinner table provide a good illustration. Such a man is not ordinarily oblivious to the table conversation, to a hair upon his plate, or (especially) to the food at the end of his fork; yet it is undoubtedly the hunger-motive that is responsible for his presence in this eating-situation.

Finally, adaptive behavior involves a response that changes the sensory situation and satisfies the motivating conditions. The pain disappears when the splinter is drawn from the

finger; the stuffiness of the overheated room is dispelled when the window is opened; hunger goes as food is ingested; the mother turns back to her pillow when the child is comforted—in all these cases an adaptive response has in some manner altered the sensory situation and removed the motivating stimulus.

In addition to these final, "consummatory" responses, which serve to eliminate the motivating conditions more or less directly, adaptive behavior shows an initial, "preparatory" phase. Consummatory responses are preceded by preparatory responses, and the latter may influence the former in a number of ways. They may (a) induce a state of readiness and alertness that helps the organism to make the final response clean-cut and effective, (b) enhance the effectiveness of the sensory stimulus, and (c) shut out distracting stimuli. Thus, (a) a low moan in the night brings me up on my elbows in bed; (b) I turn my head to listen; and (c) I check my breathing, to avoid even the faint disturbance of its sound. All these attentive reactions are made in preparation for "whatever comes." Further illustrations are unnecessary; the reader will recognize the importance of such adjustments in every situation where motivating conditions may not be immediately or directly satisfied. Even a meal, for civilized man, must be prepared for in some fashion. The actual ingestion of food—a consummatory response—is almost always preceded by reaching, grasping, leaning, and other preliminary movements, as well as those behavioral embellishments that we call "table manners."

We are about at the end of our story of functionalism as a systematic position in psychology. There is much more that might have been said. Carr's text, alone, includes a treatment of the human nervous system and sense-organs, of learning, perceiving, reasoning, affection, and volition, of individual differences and the measurement of intelligence. However, the point of view, in its broadest aspects, should

be rather clear to the reader by now. A few concluding remarks will serve to make the picture sufficiently complete for our purposes, and provide a partial résumé of what has gone before.

In spite of the fact that the functionalists professed an interest in the purely "psychical" aspect of mental activity and accepted introspection as a valid psychological method, it is quite obvious that their primary concern was in the "physical" aspect of such activity and the "objective" method of observation. Their absorption in animal, child, and abnormal psychology; their interest in "mental" tests—which, as every student knows, are really "behavioral" tests; their reverence for the Darwinian tradition; their emphasis upon the usefulness of mental activity; their treatment of the reflex-arc concept; and their notion of adaptive behavior—all these things point in no uncertain manner away from the classical, Wundtian view of psychology as the study of mind or experience, and towards a newer, biological view of psychology as the study of organismic reaction.

Besides showing a wider scope of interest than that in the normal, adult, human individual, functionalism broadened the idea of psychology's subject-matter in another respect. In rejecting structuralistic analysis and stressing the complex and continuous interplay of factors in human behavior, the men of this school displayed a willingness to accept the view, now current, that the larger, more meaningful totalities of an organism's activity are fully as important units for investigation as are the more easily measured Titchenerian elements. This is particularly evident in the adaptive-behavior concept, which dealt with a highly purposeful mode of organization. In other words, they emphasized *durational** segments of mental activity—events that took appreciable time and possessed a logical beginning and end that gave

* "Durational" as used here is to be distinguished from Titchener's durational or "protensive" attribute, a relatively shortlived affair.

them their identity. The structuralist's "determining tendency" may have been a recognition of the same thing, but this concept never reached the status for him that "adaptive behavior" did for the functionalist. Moreover, even in dealing with those momentary "cross-sections" of experience—perceptions and ideas, for instance—the structuralist seldom paid quite the attention to the *organization* of mental life that the functionalist did.

Functionalism was, at its best, a loosely knit system and school; and it is this very quality that was at once the strength and the weakness of the movement. As a system it was flexible; as a school it lacked unity. It permitted the inclusion of material from many sources (*too* many, Titchener would have said) in so far as such material was based on sound scientific procedure; but it examined no one's philosophical credentials sufficiently to avoid a confusion of tongues on fundamental issues. It had no single point of view with respect to the mind-body problem and even showed a lack of agreement as to the proper usage of the key-word "function"—matters of no great practical importance, but systematic weaknesses that functionalism's critics were not slow in pointing out.

Only in its opposition to structuralism, its accent upon biological adaptation, and its regard for utility does functionalism stand as a unified movement in the history of modern psychology. Nevertheless, its effect was, in the main, healthy, and its most recent exponents (Carr, for example) present a systematic position which is not far from being the dominant one in American psychology to-day. If its strength as a school has been hard to evaluate, this is probably due to the fact that *another* school replaced it before it really came of age.

Before the undisciplined forces of functionalism could be brought together along a single front, under one banner, the smoke and fire of a newer and more vigorous attack

upon the Titchenerian stronghold came to obscure a great deal of the advance that had been made. This new attack was that of the "*behaviorists*," under the able and aggressive leadership of a man whose dislike for the campaign measures of functionalism was only exceeded by his disdain for the guns of structuralism.

Chapter V

WATSON AND BEHAVIORISM

BEHAVIORISTIC psychology has had many names, not all of them complimentary. In addition to "behaviorism," the most common title, it has been called "objective psychology," "anthroponomy," and even "the psychology of the Other One"—not to mention such impolite labels as "muscle-twitchism." It has also had many interpreters, but the true founder and promoter of the system was John B. Watson (1878-), and it is mainly to a treatment of his point of view that I shall confine myself in this chapter.

John Watson became acquainted with structuralism and functionalism at the University of Chicago where he studied under Angell and where he received his doctorate in 1903. His dissertation was in the field of animal psychology. The experiment, now classic, that won him his degree was one in which he removed the function of one set of sense-organs after another in a group of white rats who were trained or to be trained to run successfully through a series of complicated pathways at the end of which was a reward. One conclusion that Watson drew from his experimental results was that the animals relied to a great degree upon kinesthetic cues in their mastery of such a problem—cues, that is, from the muscles themselves rather than from other sensory stimuli. It is not improbable that this conclusion had something to do with Watson's later emphasis upon muscular response in human psychology. Little straws of this sort often tell which way the systematic wind is going to blow.

At any rate Watson came to psychology with a background of functionalism and animal experimentation, and his first book, *Behavior, an Introduction to Comparative Psychology,* was dedicated, appropriately, to James R. Angell and Henry H. Donaldson (the latter an authority on the white rat). This book, which came out in 1914 while Watson was a professor of psychology at the Johns Hopkins University, and which deals primarily with animal behavior, also contained a preliminary announcement of the behavioristic program. Five years later this program was made more explicit in his *Psychology from the Standpoint of a Behaviorist,* a book entirely devoted to human psychology. Other books came later, for Watson was a prolific and popular writer, but it is in these two that we find the most carefully reasoned of his pronouncements.

"Psychology as the behaviorist views it is a purely objective experimental branch of natural science." With this sentence Watson begins his 1914 text, and follows it immediately with a brief outline of his program. Psychology is to study *behavior* as its *subject-matter;* its *method* is to be entirely *objective;* and its central *problem* is to be one of *"prediction and control."* These points constitute the positive aspect of the new system, and I shall develop them later in more detail. There was also a negative aspect of the system; it appears in Watson's criticism of structuralism and functionalism—particularly the former—and a few examples will show that he did not mince his words.

"States of consciousness," like the so-called phenomena of spiritualism, are not objectively verifiable and for that reason can never become data for science. . . . The behaviorist finds no evidence for "mental existences" or "mental processes" of any kind.

These statements are made by Watson in 1919; he used even stronger language in a later book when he said,

"Consciousness" is neither a definable nor a usable concept; . . . it is merely another word for the "soul" of more ancient times.*

In all other sciences the facts of observation are objective, verifiable and can be reproduced by all trained observers. . . . Psychology, on the other hand, as a science of "consciousness" has no such community of data. It cannot share them, nor can other sciences use them. . . . Even if they existed, they would exist as isolated, unusable "mental" curiosities.

The psychologist's use of "introspection" as its principal method has been another very serious bar to progress. . . . All that introspective psychology has been able to contribute is the assertion that mental states are made up of several thousand irreducible units like redness, greenness, coldness, warmth, and the like, and their ghosts called images, and the affective irreducibles, pleasantness and unpleasantness. . . . Whether there are ten irreducible sensations or a hundred thousand (even granting their existence), whether there are two affective tones or fifty, matters not one whit to that organized body of world-wide data we call science. †

The older psychology, says Watson, had something very esoteric in its methods. Experiments were carried out upon a very select circle of subjects, those who were trained in a highly artificial introspective technique. If a subject was unable to experience one to seven different degrees of "clearness," for example, he was judged inadequate as an observer; or if he found his "feelings" to be reasonably clear, his introspection was again at fault. "The attack is made upon the observer and not upon the experimental setting. In physics and in chemistry the attack is made upon the experimental conditions. The apparatus was not sensitive enough, impure chemicals were used, etc. In these sciences a better technique will give reproducible results. Psychology is otherwise."

* J. B. Watson, *Behaviorism*, p. 3.

† J. B. Watson, *Psychology from the Standpoint of a Behaviorist*, pp. 1, 2.

Furthermore, according to Watson, structural or introspective psychology was fast becoming so concerned with the "true" nature of mind, consciousness, sensations, and so on, that it was degenerating into a field of argumentation rather than experimentation. This deplorable state of affairs was directly attributable to its alleged subject-matter and the method of introspection.

Nor were the functionalists, in Watson's opinion, much better off than their unscientific cousins. Although they threw out the Titchenerian elements and pretended to deal with "mental functions," they still employed the introspective method. In addition, they were almost inevitably doomed to fall into some sort of philosophical dualism with respect to their subject-matter—either that of interactionism or parallelism. Concerning such positions, Watson says: "Those time-honored relics of philosophical speculation need trouble the student of behavior as little as they trouble the student of physics. The consideration of the mind-body problem affects neither the type of problem selected nor the formulation of the solution to the problem."

These are plain-spoken assertions, to say the least, but very representative of Watson. They are the statements of a young man, defiant of the dogma of structuralism, disgusted with the confusion of functionalism, eager to set psychology free from its philosophical tradition and give it equal footing with other branches of biological science. Whether they were or were not entirely justifiable statements is a question that need not bother us for the moment. Certainly they found many sympathetic ears, particularly in the younger crop of American psychologists, many of whom were already thinking, and some of whom were even writing, in the same vein, although not so forcefully.

Watson founded behaviorism in America and was, for a time, its mouth-piece and promoter. Although he established no formal headquarters comparable to the Cornell labora-

tory, or even Chicago, he was able, through the simple and colorful language of his books, his lectures, and his popular articles, to catch the fancy of a nation. If his opinions were inconsequential to some and shocking to others, they were, nevertheless, absorbed by all. Structuralism became, in the minds of many, a museum-piece, and functionalism was forgotten. His influence spread into the class-rooms of the universities and the homes of the literate laity (there were laboratory-behaviorists and parlor-behaviorists) and it is with some reason that, even to-day, for many Europeans, American psychology and Watsonian behaviorism are indistinguishable.

No attempt will be made, in what follows, to discuss the systematic views of any behaviorist other than Watson. The aim in this context, as previously, is to present our major problem—defining psychology—in terms of individual theorists who had vivid ideas on that subject. Such a procedure is questionable, since it focuses attention upon men, rather than the broader developments in which they merely played a conspicuous part; but it is hoped that what is lost of generalization may be returned in the coin of specificity—that it will be better for the reader to have a rather accurate account of Watson's position than a hazy interpretation of an entire movement.

In 1919 Watson defines psychology as "that division of natural science which takes human behavior—the doings and sayings, both learned and unlearned, of people as its subject matter." He qualifies and expands this general statement by saying that behavior consists of "responses," "reactions," or "adjustments" of an organism to certain antecedent events—"stimuli" or "stimulus-situations." We are already familiar with "stimulus" and "response" from our discussion of functionalism. The terms were originally physiological, and Watson points out that we must extend their meaning somewhat in psychology to cover more com-

plex, integrated events than those with which physiology ordinarily deals.

We might, he argues, use "stimulus" to designate such simple, measurable things as light-rays or sound-waves, and "response" for such restricted activities as movements of a muscle or group of muscles; while "stimulus-situation" and "adjustment" might be employed with reference to more complex matters—one's surroundings at a given moment, and one's talking, walking, or other activities. However, "stimulus" and "response" are terms satisfactory enough for either order of complexity, if we remember that we have extended the physiological usage. This is justifiable, moreover, on the grounds that the more complex events of either sort are theoretically analyzable into their simpler components: "a situation is, . . . upon final analysis, resolvable into a complex group of stimuli," and an adjustment includes a "whole group of responses . . . integrated in such a way . . . that the individual does something which we have a name for."

Watson's primary interest is in the *response* factor, since that is characteristic of the organism itself,* and he shows us two ways of subdividing this category. Physiologically, responses involve the action of "effectors"—muscles and glands. The striped or skeletal muscles are responsible for all external movements of an organism in space; the unstriped or smooth muscles are concerned with the organism's internal adjustments. The activity of the former is seen in most of our everyday responses, from the crooking of a finger to the rowing of a boat. That of the latter is seen in the responses of the stomach, bladder, blood-vessels, and the like. The responses of the glands include the secretion, under appropriate conditions, of tears, sweat, saliva, and so forth.

Psychologically, the responses of these muscles and glands

* Watson did not, of course, ignore the fact that some stimuli, e.g., the kinesthetic and organic, arise within the organism.

may be conveniently grouped in four main classes. There are (1) "explicit habit responses," (2) "implicit habit responses," (3) "explicit hereditary responses," and (4) "implicit hereditary responses." "Explicit" and "implicit" are synonymous, respectively, with "overt" and "covert" or "observable" and "non-observable." Within the first class are most of those work and play activities in which humans are daily engaged—"unlocking a door, tennis playing, violin playing, building houses, talking easily to people, staying on good terms with the members of your own and the opposite sex"; in the second class are those smooth-muscle and glandular reactions that have been established in us through some degree of training—blushing at the sight of a loved one, sweating at the sound of a dentist's drill; in the third class are "man's observable instinctive and emotional reactions as seen, for example, in grasping, sneezing, blinking and dodging, and in fear, rage, love"; and in the fourth class are included the various gland secretions, circulatory changes, and the like that have been studied in such detail by the endocrinologists and physiologists.

Watson devotes several chapters of his text to an account of these response-classes, and we shall have occasion to refer to them in more detail when we consider one or two of the prominent doctrines of his system. For the present, however, we had better employ ourselves with his treatment of the *methods* of psychology.

The *general* method, as mentioned above, is that of objective observation, but there are many *specific* objective techniques—experimental and otherwise—that the psychologist utilizes in his work. These, upon examination, show themselves to belong under one of four main headings: (1) observation, with and without experimental control; (2) the conditioned-reflex methods; (3) the verbal-report method; and (4) the methods of testing.

The first class includes all those necessarily crude ob-

servations of man in his everyday world, together with the more refined observations of the laboratory. Thus, we may judge from a subject's reactions to the objects of his common environment that he has some visual or auditory weakness; but only when we isolate, repeat, and vary the conditions of an observation systematically (in other words, perform an experiment) can we arrive at any definite and quantitative statement of the subject's defect. Apparatus and refined techniques (Watson did not scorn "brass-instrument" psychology) should be employed wherever possible to increase the delicacy of our judgments and extend the power of our senses; laboratory-study should supplement field-study.

The conditioned-reflex methods really constitute special examples of the way in which instruments are used in psychological observation, and Watson's emphasis upon them is illuminative of his scientific background and his "objective" leanings. These techniques originated in the laboratory of the Russian physiologist, Pavlov. They became the chief basis for a program of experimental studies of animal behavior, particularly dog-behavior, where no verbal report was involved, and where the conditions of observation were highly objective and carefully controlled. Watson proposed to apply conditioned-reflex methods to studies of human behavior as a check upon, or substitute for, less objective methods, and in those investigations where language methods are impossible or inadvisable (studies upon infants, the deaf and dumb, and certain pathological subjects).

Examples of *simple* reflexes have already been given in the treatment of functionalism, but two or three more are now in order. When food is placed in a hungry man's mouth, saliva will begin to flow; when a strong electric shock is applied to a man's finger-tip, the finger will be withdrawn. These are "natural" responses to stimuli and they may be called "*un*conditioned reflexes." If, now, we supply the

sound of a buzzer just before the presentation of the food (or the application of the shock) on a number of successive occasions, our subject will begin to salivate (or withdraw his finger) at the sound of the buzzer—even when this sound is not followed by food (or shock). The buzzer acts as a *substitute* for the "natural" stimulus.

The general term for such a modified or acquired stimulus-response relationship is a "conditioned reflex," and we call the substitution-procedure "conditioning." We speak of a "conditioned secretory reflex" when the response is a glandular one, and of a "conditioned motor reflex" when it is muscular. The conditioned-reflex methods of Watson's classification involve the establishment, in human subjects, of certain conditioned reflexes—with the purpose of finding out something about the subject's response-capacities.

If the muscular or glandular response of an experimental subject can be conditioned to the sound of a given tone, when all other possible "cues" for his response are eliminated, we may be sure that he is sensitive to such a sound —that he "hears" the tone. With this fact determined, we may proceed up or down the scale of tonal stimuli, by little steps or great, until we learn the *range* of his sensitivity. Moreover, by presenting the unconditioned stimulus (food or shock in our examples) in connection with one tone and withholding it in connection with other near-by tones, we may determine the least noticeable tonal difference that our subject may sense anywhere on the scale. We may find, in other words, the *sharpness* of his sensitivity. And all this may be done without requiring a single word of report from our subject; in fact, when we use the conditioned *secretory* reflex, he may not even know how sensitive he is to these stimulus-differences.

A word about terminology is appropriate in connection with Watson's third class—the verbal-report methods. The "observer" of Titchener's experiments is the "subject" of

Watson's. Introspective psychology called for an experimenter *and* an observer in laboratory studies. The experimenter set up the essential conditions and the observer reported upon his "experience," a report that was recorded, of course, by the experimenter. In behavioristic studies the true observer *is* the experimenter, who observes, not his "experience," but the *responses* of his subject. The essential subject-matter, in the former case, is that which the "observer" describes—namely, his "experience"; in the latter case it is the "observer's" *description,* i.e., the verbal reactions that the subject makes under the conditions prepared by the experimenter.

The verbal-report method is, therefore, the Watsonian substitute for the method of introspection. The difference between the two is a difference of interpretation rather than of actual practice. When, for purposes of expediency, the rather laborious and time-consuming conditioned-reflex methods are not to be recommended, or when the responses cannot be registered other than in the subject's words, the verbal-report methods may be of value. However, because of their inexactitude, they should be used as sparingly as possible.

The *testing methods* need no elaboration here. Watson considers as "behavioral" the various "mental" tests—intelligence tests, special-ability tests, and the like—since the psychologist's real interest, in every case, is in the "doings" (verbal, manual, and otherwise) of a subject with respect to certain test problems. He points out that more emphasis should be placed upon the non-language tests and that we need not treat the tests as unscientific merely because they find so much application in the everyday pursuits of human beings.

The *problem* of psychology, as mentioned above, is the prediction and control of human behavior. Or we may say that psychology is confronted with *two* problems: "the one

of predicting the probable causal situation or stimulus giving rise to the response; the other, given the situation, of predicting the response."

Here, perhaps better than elsewhere, we see the reason for the wide appeal of Watson's system. Most human beings would like to know how to predict and control the behavior of others—"to take advantage of others" might be the appropriate phrase in some instances. What reader of this book, for example, would not be willing to spend many hours at his "psychology," if he were certain thereby to enhance his power of social control?

Watson could have said, without greatly misrepresenting his program, that psychology's task was to analyze human behavior into reflex elements, to study the laws of connection of these elements, and show the nature of their dependence upon neural function. He chose, instead, the prediction-control motif—a motif that every one regards as a distinct feature of exact science, and one that has been attractive to man since time immemorial.

In dealing with these problems, Watson points out that, in order to predict the probable causal situation from our observation of some behavioral item, many facts must be at our disposal. For example, the answer to the question "Why do men go to war"? requires a knowledge of (a) man's unlearned response repertory, (b) the various habits he has formed, (c) the tradition of the group to which he belongs, (d) the social conventions he respects, and (e) the effect of such agencies as the church and the school upon his development. Furthermore, these facts themselves can only be acquired through the prolonged application of psychological methods to the study of human behavior from infancy to old age—the so-called "genetic" or "developmental" approach.

The other face of the problem—"given the situation, to predict the probable response"—is equally difficult of solu-

tion and demands equally a background of psychological knowledge. But the fundamental answer is the same. We must amass behavioral data through the untiring use of our experimental procedures; and, eventually, such data will permit us to reply, in other than the present hit-or-miss fashion, to such broad social questions as might be raised concerning the probable effect upon human initiative of a certain form of government, or such individual questions as the effect of sudden wealth upon a friend.

This is a large order for psychology, and Watson realized it, but his faith in the experimental and genetic approach to the study of human conduct led him to believe that much could be done in this direction. By studying, in the laboratory whenever possible, the behavior of the infant, the child, the adolescent, the adult, and the senescent—both normal and abnormal—he hoped that psychology might be able to offer expert advice in many practical spheres of human activity.

He was, moreover, not slow in leading the way. His stress upon the genetic method and his interest in both the "pure" and "applied" aspects of science are seen clearly in his own selection of experimental problems. One of the best known of his investigations may be considered here as typical of the program that he sponsored.

The distinction between hereditary and habitual responses has already been made, and it has been noted that Watson gave both types extensive consideration in his 1919 text. He was not the first to draw a line between the two. The problem of the "innate" *versus* the "acquired" was old when John Locke was a boy and René Descartes was writing about "innate ideas." But Watson brought to the study a relatively new point of view. He proposed to settle the question by appealing, not to the behavior of the adult human, but to that of the newborn child—tracing the development

of reaction step by step and noting the first appearance of hereditary as well as acquired modes of response.

Scientific studies of infant-behavior are not always easy to carry out, mainly because of the objections raised by adults, and parents in particular. Moreover, the validity of some conclusions drawn from such studies has been roundly challenged because of the small number of subjects experimented upon. It is usually unsafe to generalize about the reactions of infants in general from observations made of a single child, or even a dozen.

Watson, while at Johns Hopkins, was able to surmount both of these obstacles by obtaining permission to examine psychologically (as a part of regular hospital routine) several hundred newly born babies at the Harriet Lane Hospital in Baltimore. By means of systematic and almost daily observations it was possible to catalogue, at the time of their appearance, the unlearned responses of infants during the first months—in some cases, years—of life.

Able assistance and a wide range of tests enabled him to collect a great deal of factual material of this sort, a complete enumeration of which need not be undertaken here. In brief, he found a large repertory of reflex activities (sneezing, crying, grasping, blinking, and so on) that appeared in rather well-defined sequence during the early days of infancy; and three types of emotional response (rage, fear, and "love") that "belong to the original and fundamental nature of man."

With respect to these emotional responses, Watson argued that they were each elicited at their first appearance by a restricted number of stimuli. Loud sounds and loss of support evoked the fear response. The blow of a hammer upon a steel bar or the sudden jerk of a blanket from beneath a half-sleeping infant were very effective. The hampering of an infant's natural movements evoked rage. The soft strok-

ing, patting, or manipulating of certain sensitive bodily zones were effective in bringing out the "love" response.

As to the behavior itself, in these fundamental emotions, Watson reported that there were three discernible "patterns" of response. Fear involved the catching of the breath, tight closure of the eye-lids, random clutching movements of the hands, puckering of the lips or crying, and other observable changes. Anger or rage involved such responses as the stiffening of the infant's body, slashing movements of the hands and arms, holding of the breath. While love (Watson asks us to strip the word of its old meanings) showed itself in smiling, gurgling, cooing, and, in older children, extension of the arms—"the forerunner of the embrace of adults."

The more specialized and coördinated emotional display of adults was attributed by Watson to the development and elaboration of these unlearned patterns of infancy; and he maintained that the great variety of objects and situations known to call out emotional reactions in later life were to be explained by reference to the principle of "conditioning." He found, for example, that, in spite of much baby lore to the contrary, no fear was shown by infants when presented for the first time with cats, dogs, pigeons, rabbits, and a number of animals at the zoo. This lack of fear response was due, in his opinion, to the fact that the children had not *learned* to be afraid of these things. Even fear of the dark, so common in children, he traced to past experience with loud sounds in the absence of light: "A child that has gone to bed for years without a light with no fears may, through the loud slamming of doors or through a sudden loud clap of thunder, become conditioned to darkness." (John Locke, in talking of the "association of ideas," said something in a similar vein: "The ideas of goblins and sprites have really no more to do with darkness than light; yet let but a foolish maid inculcate these often on the mind of a child, and raise them there together, possibly he shall

never be able to separate them again so long as he lives; but darkness shall ever afterwards bring with it those frightful ideas, and they shall be so joined, that he can no more bear the one than the other.")

In order to test the validity of this explanation, Watson carried out experiments to determine the possibility of conditioning the fear-pattern to other than the "natural" stimuli. In one of these investigations he used as a subject an eleven-months-old infant—"stolid and phlegmatic, but extremely well and healthy"—and combined the stimulation of a loud sound and a white rat, on a number of successive occasions. The child's original response to the rat was of a friendly, investigatory nature; but, after no more than seven combined presentations of rat and loud sound, the sight of the animal alone was sufficient to evoke a strong fear-reaction. "The instant the rat was shown the baby began to cry. Almost instantly he turned sharply to the left, fell over, raised himself on all fours and began to crawl away so rapidly that he was caught with difficulty before he reached the edge of the table."

Moreover, there was a *transfer* of this emotional reaction to a large number of other stimuli. The presentation, a few days later, of a rabbit, a dog, a fur coat, cotton wool, human hair, and a Santa Claus mask was decidedly fear-inducing —in spite of the fact that the infant had previously played with each of these objects at every opportunity.

It is through this mechanism of conditioning and transfer, Watson argued, that human beings may be provided, during infancy, with much of their complex emotional equipment—sometimes to their serious disadvantage. All those failures in adult social adjustment—ungrounded fears, unwarranted rages, and unreasonable love-attachments—may have their roots in the emotional upsets and accidents of infancy or early childhood. Likewise, all of the more complicated emotional responses common in adult behavior—to

which we give such names as "shyness," "shame," "hate," "pride," "jealousy," and "anguish"—are combinations and permutations of the three elemental response-patterns of fear, rage, and love. We may note, in passing, that Descartes, more than two centuries earlier, maintained that there were *six* primary emotions—wonder, love, hate, desire, joy, and sadness—from which all others evolved; but his classification, like many others before and since, lacked the sanction of experimental and genetic studies.

By this time the reader has perhaps wondered what became of Watson's "stolid" youngster with the "transferred" fears; or, what is more to the point, whether emotional responses of this sort can be removed as well as implanted. Three years after the studies just reported Watson undertook to answer this question experimentally with another group of children. In this case he selected subjects of different ages, in whom there appeared pronounced fear-responses, of unknown origin but presumably conditioned, and attempted to "*un*condition" them.

The procedure employed in the eradication of conditioned fears was somewhat similar to that used in their fixation. An example will show this. In one experiment Watson chose as his subject a three-year-old boy in whom there was excellent adjustment to ordinary life situations, except for an exaggerated fear of white rats, rabbits, fur coats, and the like. This child was accustomed to taking his afternoon lunch of crackers and milk in a room about forty feet long. In the tests he was placed at one end of this room, with his lunch before him. At the *same time* a rabbit, in a wire cage, was displayed to the child "just far enough away not to disturb his eating." This place was marked by the experimenter and, on succeeding days, the rabbit was brought closer and closer to the subject—in each case almost to the "disturbing point." Eventually the child came to eat with one hand and play with the rabbit with the other! In addition, his emo-

tional response to other fear-objects was markedly diminished and, in some cases, entirely eliminated—another example of a "transfer" effect. No scolding, no coaxing, no compulsion was involved; the careful application of an experimental technique was sufficient.

Watson had, of course, some theoretical statements to make about emotions and their traditional partners, instincts. He defined emotion as "an hereditary 'pattern-reaction' involving profound changes of the bodily mechanism as a whole, but particularly of the visceral* and glandular systems." Stated in terms of the four response-categories mentioned above, emotion is basically a matter of "implicit hereditary response" and "explicit hereditary response," with the former predominant.

"Instinct" is treated, in 1919, as similar to emotion and usually concurrent with it, but more "explicit" (observable), and more adaptive (i.e., better suited to adjust the organism to its external environment). It is also less chaotic in its onset, and less diffuse in its nature. It is defined as "an hereditary pattern reaction, the separate elements of which are movements principally of the striped muscles." (Five years earlier Watson had defined it similarly as "a series of concatenated reflexes," a definition not unlike Herbert Spencer's description of instinct as "compound reflex action.")

Watson's examples of instincts in human infants may, however, be taken as equally good illustrations of reflexes, for he includes such responses as "sneezing," "yawning," "stretching," and "nursing." His discussion of the matter emphasizes the fact that instincts are practically impossible to identify in adult activity because of the over-layer of habit-responses that hides them from observation. It is not surprising that, later, in his popular book, *Behaviorism* (1924), he boldly discards these "hereditary pattern-reac-

* "Visceral" refers to the reactions of the large internal organs (viscera) of the body—the heart, stomach, intestines, and so on.

tions" and affirms that all of the complex organized behavior of adult humans is a result of the influence of training (conditioning) upon the basic, unlearned beginnings of response (reflexes). Even such early activity as that of crawling and walking is described as a conditioned development of the reflex responses of the arms, legs, and trunk of the child; and handedness (right or left) is treated as a learned, rather than an hereditary, characteristic. Furthermore, it is emphasized that the emotions themselves, although pattern reactions initially, are soon conditioned in many ways that obscure their early unlearned nature.

Especially does Watson assail, in this book, the idea of inherited "capacities," "talents," "temperaments," and the like. The musician's son who takes up music, the criminal's son who turns to crime, the John Stuart Mill who follows in his father's footsteps, the "chip off the old block" in every walk of life—all are the result, not of hereditary, but of *environmental* factors. Says Watson:

> Give me a dozen healthy infants, well formed, and my own specified world to bring them up in and I'll guarantee to take any one at random and train him to become any type of specialist I might select—doctor, lawyer, artist, merchant-chief and, yes, even beggar-man and thief, regardless of his talents, penchants, tendencies, abilities, vocations, and race of his ancestors.

This proposal to throw out instinct from psychology came as a shock to many of Watson's readers. Some, who liked its implications, were heartily sympathetic; others, who did not, were equally antagonistic; but both groups were impressed. They were so impressed that, to-day, in the minds of many, John Watson, behaviorism, and the denial of instinct are all one; and this is in spite of the fact that there are many behaviorists other than Watson, and very few of them would agree with him in detail on this or other topics.

Watson had other startling things to say—other novel proposals with reference to age-old psychological problems. One of these deserves attention here because it illustrates further the scope of his system and answers a question that might seem, to the reader, unanswerable from a behavioristic platform. The problem is that of the nature of *thought;* the question, "What is thinking"?

"Thinking," in the Watsonian scheme, falls under the heading of "implicit habit response" and is primarily a matter of "language" activity. Language, however, is not entirely confined to vocal responses (laryngeal or "voice-box" reactions), but includes movements of a gestural sort —for example, the shrug of the shoulders, the wave of the hand, the wink of an eye. Thinking differs from talking in being implicit rather than explicit, covert rather than overt; it is, in a very real sense, "sub-vocal" talking and "sub-gestural" gesturing, and it may even involve widespread reactions, provided that these reactions have become conditioned to serve as substitutes for other reactions or stimuli.

Watson offers various types of evidence to support this position. The child who "thinks out loud," but who is reduced by parental admonition first to "whispered" and finally to "silent" response; the none-too-socialized persons who, when alone, whisper or speak their "thoughts" or move their lips in reading; the deaf-mute who talks and thinks (even dreams) with his fingers—all these may be taken as proof that thinking and talking (or gesturing) are one and the same.

Watson presents other evidence, often of a direct, experimental nature, to show that implicit response and thought may be identical, but the inclusion of this material here would bring us into a controversial field and certainly would not alter *his* view of the matter. Neither need we consider his treatment of the various stages or degrees of thinking—from the mere unwinding of language habits to the trial-

Watson had other startling things to say—other novel proposals with reference to age-old psychological problems. One of these deserves attention here because it illustrates further the scope of his system and answers a question that might seem, to the reader, unanswerable from a behavioristic platform. The problem is that of the nature of *thought;* the question, "What is thinking"?

"Thinking," in the Watsonian scheme, falls under the heading of "implicit habit response" and is primarily a matter of "language" activity. Language, however, is not entirely confined to vocal responses (laryngeal or "voice-box" reactions), but includes movements of a gestural sort —for example, the shrug of the shoulders, the wave of the hand, the wink of an eye. Thinking differs from talking in being implicit rather than explicit, covert rather than overt; it is, in a very real sense, "sub-vocal" talking and "sub-gestural" gesturing, and it may even involve widespread reactions, provided that these reactions have become conditioned to serve as substitutes for other reactions or stimuli.

Watson offers various types of evidence to support this position. The child who "thinks out loud," but who is reduced by parental admonition first to "whispered" and finally to "silent" response; the none-too-socialized persons who, when alone, whisper or speak their "thoughts" or move their lips in reading; the deaf-mute who talks and thinks (even dreams) with his fingers—all these may be taken as proof that thinking and talking (or gesturing) are one and the same.

Watson presents other evidence, often of a direct, experimental nature, to show that implicit response and thought may be identical, but the inclusion of this material here would bring us into a controversial field and certainly would not alter *his* view of the matter. Neither need we consider his treatment of the various stages or degrees of thinking—from the mere unwinding of language habits to the trial-

and-error solution of difficult problems. The general doctrine should be clear: thinking is response; the response is predominantly verbal; and its final result is adjustment of the human organism to a complex social environment.

We have already considered the Watsonian system with respect to the subject-matter, the methods, and the problems of psychology; and we have sampled its investigations and concepts. The remainder of this discussion will be in the form of broad characterizations aimed at giving the reader an opportunity to compare the system with those I have previously outlined.

First of all, behaviorism, in the hands of its founder, tended to stress the "cross-sections" of human conduct rather than what I have called the "durational" aspect. In spite of a great deal of emphasis upon genetic studies and the "past history" of the organism, and in spite of frequent references to the organization of successive responses in thinking and other activity, Watson's detailed behavioral descriptions were generally descriptions of relationships between stimuli and responses that were closely joined in time.

In this respect the system resembles structuralism—with its accent upon momentary mental contents or processes. There was little in the Watsonian portrayal that might have been likened to the functionalist's stress upon stimulus-response interplay or the concept of "adaptive behavior." The bodily "set," "orientation," or "attitude" had not yet become a characteristic tenet of behaviorism, even to the degree that Titchener's "determining tendency" was a feature of structuralism. "Urges," "drives," "motives," and "organic needs," which figure so prominently in the literature of to-day's psychology, were scrupulously ignored by Watson, possibly because of their historical association and identification with the instinct concept.

Secondly, like Titchener, Watson was an elementarist in

psychology. He held that the complete personality of an individual is built up from a large number of very simple unlearned response-elements through the process of conditioning; just as Titchener argued for mind as a fusion and combination of mental elements, joined together, at least in great part, through the principle of association. This elementarism was not so explicit in Watson as in Titchener because of the former's emphasis upon the end-appearance of such combinations—the integrated, response-groups rather than the members of such groups—but it is obviously a significant phase of his systematic position.

Thirdly, notwithstanding the fact that, in Watson's 1919 text, he presented a rather elaborate account of the nervous system from a purely anatomical and physiological point of view, it is clear that he was not so much interested in the more or less hypothetical "mechanism" of behavior as he was in the behavior itself—the observable acts and adjustments of the organism with respect to its environment. In 1924 he states in outright terms that the nervous system has been little more than a "mystery box" for the introspective psychologist: "whatever he couldn't explain in mental terms he pushed over into the brain." Watson proposes to give this system no more status than any other part of the response-mechanism—in his own words, "to place no more emphasis on the brain and spinal cord than upon the striped muscles of the body, the plain muscles of the stomach, the glands, etc." Moreover, he points out that the adjustments of the *whole* organism are more significant for the behaviorist than the workings of *parts;* and his distinction between the behaviorist and the physiologist is based largely upon the latter's concern with specific bodily functions such as digestion, respiration, and circulation, whereas the former "works with the whole body in action."

Finally, the practical slant of this system exceeds even that of functionalism. This should already have been evi-

dent to the reader in the discussion of "prediction and control." The questions that Watson posed for psychology were always questions of a common-sense nature, the answers to which every one would like to know. Moreover, he conceived it to be the duty of experts in psychology to lend hands in many places—in advising parents, educators, lawmakers, and business-men; in helping the individual organize his own life activities and in helping society progress in its knowledge and control of the individual. In contrast with Titchener, who described the scientist as disinterested and impersonal—devoted to facts rather than values—Watson never divorced observation from application; he was as ready to use a fact as to find one. Titchener would have called Watson a "technologist" rather than a pure scientist; and Watson would not have minded!

Many arguments have been launched against behaviorism and many epithets have been hurled at its founder. The system has been called "materialistic" and "mechanistic" because it had no place for "consciousness," "mind," "soul," "volition," and kindred concepts; and because it sought a natural-science explanation of psychological matters. It has been criticized as crude, illogical, naïve, and subversive—even "anti-psychological." Watson himself has been accused of over-simplifying psychological problems; of forcing mental categories into physical pigeon-holes; of producing theories that could not be verified; of supporting an untenable position in the eyes of philosophy, medicine, law, religion, and ethics; of making art meaningless; and of corrupting the youth.

Into the truth or falsity of these accusations we need not here inquire. Some of the issues raised were pointless and irrelevant; others were not, and will receive some consideration in a later chapter of this book. There is a warlike literature that the reader may consult if he desires, but it

is not always an enlightened literature. Strong emotion does not further the best in evaluative judgment, and Watson elicited many angry responses.

However, it should not be assumed that Watson lacked supporters. The philosophers, psychologists, religionists, and writers who assailed behaviorism were met by other, equally aggressive gentry who came to its defence. It was as often hailed as a panacea as it was a poison, with as much (or as little) justification. The real strength of a system, however, is not measured by the heat of its first skirmishes or the clamor of camp-followers.

The heyday of Watsonian behaviorism was exciting, but it could not last for long. The freshness of its appeal was bound to fade in the public eye as the public fancy turned to other things, and as serious-minded students of psychology began to examine the system in a contemplative fashion. Other behaviorists, more academically-disposed contemporaries and successors of Watson, came forward to advance the general program; but there were many cautious reservations and well-considered modifications. The viewpoint, under the pressure of its critics, was clarified, qualified, and broadened in its scope. Now, in its tailored form and in more sober colors, it nearly suits the customer for whom it was intended—the experimental psychologist of the twentieth century.

Whether behaviorism grew to its full size, blossomed, and then withered, may be a matter for historians to settle. Whether it absorbed psychology or was absorbed by psychology is, perhaps, a question for the future. Whether its immediate effect upon psychology would have been greater without the advent of another, strikingly different, but equally disturbing point of view will never be known. My own judgment on these issues will have to be postponed—at least for another chapter—while attention is given to a

system that grew up with behaviorism and whose subtle, foreign charm and dramatic qualities for the time nearly eclipsed all other attempts to formulate a definition of our science.

Chapter VI

GERMANY AND GESTALT

IN 1912, Titchener, the structuralist, had just seen his *Textbook* printed in German; Angell, at Chicago, was publishing his third exposition of functionalism; and Watson, at Johns Hopkins, was preparing to shout the battle-cry of behaviorism. In that year there appeared in a German psychological journal a report, from the University of Frankfurt, of some "Experimental Studies of Apparent Movement." The writer was Max Wertheimer (1880-), and the two principal observers in the experiments he described were Wolfgang Kohler (1887-) and Kurt Koffka (1886-). The report itself marks the beginning of a new psychological system—a system of which Wertheimer was to be called the founder, and Köhler and Koffka the chief exponents.

"Gestalt" psychology is a purely German product and we had best adopt the German name for the school.* Translations of "gestalt" are sometimes employed—"shape," "pattern," "form," "structure," and "configuration" have all been suggested as English equivalents—but none have been quite so acceptable as the German word itself, for reasons that require no presentation here.

Like functionalism and behaviorism, Gestalt psychology arose in the form of a complaint. Functionalism complained about structuralism; behaviorism complained about struc-

* In this text the word "gestalt" will be capitalized only when it refers directly to the *school* or *system;* and "gestalten" will be used as the plural of "gestalt."

turalism *and* functionalism; and Gestalt found fault, to a greater or less extent, with the concepts of structuralism, functionalism, *and* behaviorism. It seems as if the remark of David Hume is no less applicable to-day than it was in 1739: "Nothing is more usual and more natural for those, who pretend to discover any thing new to the world in philosophy and the sciences, than to insinuate the praises of their own systems, by decrying all those, which have been advanced before them." Certainly Gestalt psychology did much decrying, particularly with respect to structuralism and, a little later, behaviorism, and there were two or three principal hypotheses that it attacked with especial vigor.

The first protest was against the doctrine of elementarism in psychology—the belief that mind (or behavior) is a mere collection, mosaic, or "bundle" of elementary units. A doctrine of analysis into elements we have already observed in Locke, Hume, and the Mills—father and son; in Wundt and Titchener; and, to a degree, in Watson. The nature of the elements subscribed to have varied from Locke's "ideas" to Watson's "reflexes," but with practically no exceptions, theorists have favored some form of elementary analysis since the very beginnings of psychology.

The Gestalt attack was levelled primarily at that form of "bundle hypothesis" sponsored by Titchener and, less explicitly, by Watson. The charge against Titchener was that sensations, images, and feelings are *not* the raw materials out of which mind is built, but are the products of the highly sophisticated technique of introspection, which destroys the object of description to leave a handful of elements "which nobody ever sees." Watson was similarly condemned on the ground that behavior is *not* a combination of reflexes, either original or conditioned, but that these units are established through "bit by bit" observation which has regard only for easily registered but insignificant facts, leaving the more important aspects of activity untouched.

The second major complaint of the Gestalt school was against the "associationistic thesis"—a doctrine that developed alongside of elementarism and was closely related to it. Association, as we have seen, supplied the mental glue that held the elements together—the mortar for the psychic bricks. Since, to the Gestalt psychologist, the elements themselves were artificial, the connections between the elements were bound to be equally so. Their criticism was aimed not only at the mental mechanics of James Mill but the mental chemistry of his son, and all of the later modifications of the doctrine—even the behavioristic principle of conditioning. The *organization* that characterizes all experience or behavior was *not,* according to these critics, to be explained by appeal to connections or bonds. The whole problem was, to them, a false one, arising from a mistaken view of the nature of analysis.

Some of the logical and experimental justification for these criticisms (and related ones) will be clearer as we proceed. There are two outstanding systematic treatments of the Gestalt point of view—Kohler's *Gestalt Psychology* (1929) and Koffka's *Principles of Gestalt Psychology* (1935)—both of which contain many assaults upon rival schools and many specific examples of their failure to meet psychological reality. Gestalt is a closely knit school and these two texts are in rather complete accord on fundamental issues.

Neither book, however, is easy reading, even for the specialist in psychology. The terminology is markedly different from that of structuralism, functionalism, or behaviorism, and the point of view, coming out of a philosophical and scientific tradition that I have been compelled to neglect in this historical approach, may seem disturbingly novel to the beginning student.

Psychology is defined by Koffka as *the study of behaviour in its causal connection with the psychophysical field.* This definition, however, requires considerable qualification if it

is to be distinguished adequately from those with which we have already concerned ourselves in this book.

First of all, there are to be differentiated two kinds of behavior: "molar" and "molecular." The simple reflexes of Watson's studies—what Koffka calls "relatively isolated movements elicited by relatively isolated stimuli"—and the reflex arcs of the nineteenth century physiologists—those rather sharply defined nerve-paths over which an excitation passes from sense-organs to muscles or glands—are considered by Koffka as examples of *molecular* behavior. *Molar* behavior, on the other hand, is taken to include such things as riding a bicycle to work, attending a football game, or carrying on a flirtation.

This distinction between molar and molecular may remind the reader of Watson's distinction between "adjustment" and "response"; but Koffka points out that molar behavior is by no means to be thought of as analyzable into a number of molecular elements. Furthermore, a fundamental difference (for Koffka) between molar and molecular behavior is that the former takes place *in an environment*, while the latter occurs *within an organism* and "is only initiated by environmental factors called the stimuli."

Psychology is to study molar behavior; and molar behavior occurs in an environment: the bicyclist wheels along the avenue; the football fan's activity takes place in the crowded stadium; and the flirtation occurs in a suitable, essentially social, atmosphere. Moreover, *two* kinds of environment may be distinguished, in but one of which, strictly speaking, behavior may take place. There is a *geographical* and a *behavioral* environment; and Koffka tells us a story to illustrate the difference between them.

A lone horseman, according to a German legend, arrived one wintry night, after long hours of riding over a wind-swept and snow-covered plain, at the door of an inn where he sought warmth and shelter. The innkeeper, obviously

surprised to see him, asked him from what direction he had come. The traveler replied by pointing out his route. The innkeeper, more astounded than ever, told him that he had ridden across the thinly frozen Lake of Constance; whereupon the traveler, overcome by the shock of the discovery, fell stone dead at the innkeeper's feet.

Geographically, says Koffka, the traveler of this legend rode across a lake; behaviorally, he crossed a plain. In everyday language the horseman "thought" he was riding on solid ground, but "really" he was passing over thin ice. His behavior was behavior-with-respect-to-a-plain, rather than behavior-with-respect-to-a-lake; the Lake of Constance was his geographical, the plain was his behavioral, environment. The behavioral environment, although determined in part by the geographical environment, was not identical with it. One might even say that it was the geographical environment taken *from the traveler's point of view.*

What holds for Koffka's legendary horseman would hold for all of us in all of our activities. Except in very rare cases, our behavior is regulated directly by a behavioral environment, and only indirectly by a geographical one. The bicyclist may ride along the same (geographical) avenue as the motorist who passes him; the loyal fan who cheers loudly for one football team may share his bench with an equally ardent supporter of the other, and the partners in flirtation may "carry on" in the same drawing-room. In each case the two behavioral environments have much less in common than the geographical, and they are much more significant with respect to the reactions that are evoked.

The reader who has followed this carefully will perceive that Koffka's behavioral environment is not Watson's "stimulus-situation," but comes closer to being Titchener's "experience." A glance back at Koffka's definition of psychology —which shows that behavior is to be related to a "psychophysical field"—will also make it clear that this behavioral

environment, since it "causes" behavior, must have something of the nature of the psychophysical field.

The behavioral environment *does,* for Koffka, constitute an important part of the psychophysical field, but the latter really includes a great deal more. It comprises, in addition to the behavioral environment, the awareness of certain other things: "desires and intentions, . . . successes and disappointments, . . . joys and sorrows, loves and hatreds," as well as one's own actions. To return to our horseman: besides his knowledge of the behavioral "plain" and his other "external" surroundings, there was, perhaps, a desire to press on, an annoyance at having lost his way, and an awareness of the stiffened, cramped action of his own muscles and joints as he sat in his saddle. Only when we add this material to the behavioral environment do we have the totality of a subject's consciousness or, as Kohler has called it, "direct experience."

It must now be shown why even direct experience is not the complete equivalent of the psychophysical field of Koffka's definition. This will amount to a demonstration that there are other causes of behavior than those of which a person is conscious; and I shall select three examples to make the point for Gestalt—three specimens of behavior that have no conscious determinants.

(1) If, into the eyes of a boxer knocked unconscious, a bright light is thrown, the pupils of the eyes will still contract. (2) A bachelor, once jilted within the hearing of his own wedding bells, has a strong dislike for bells, which he traces to their musical imperfection. (3) An experienced telegrapher, with his ear to a sounder, copies a message on the typewriter before him, while indulging in a friendly chat with a fellow-employee.

These are samples of three types of behavior-determinants considered by Koffka as being outside the direct experience of the individual. The boxer did not know that the light

was causing his pupillary reaction, nor would he have known it if he were "conscious"; the bachelor's explanation of his dislike for bells convinces no one that the real cause was not his being left at the altar; and the telegrapher's ability to carry on two tasks at once is not to be attributed entirely to rapid fluctuations of attention, especially since he may be unable to report the content of the message copied during the conversation with his friend.

Reflexes, unconscious determinants, and certain aspects of memory and skill—these are the three classes into which our three examples fall. They are types of behavior that demand the inclusion, within the psychophysical field, of more than conscious forces. If the reader wonders why reflexes, previously grouped as molecular behavior, are mentioned here, it is because Koffka finds a place for them, as "field-determined," in his system. The complete field will consist of "external experiences" (the behavioral environment), "internal experiences" (desires, intentions, etc.), and other forces that have no status in experience at all.*

Koffka's definition requires still more elaboration. The Gestalt psychologist argues that direct experience itself is closely paralleled by neural, especially brain, activity; the organized consciousness of a person is a true representation of a corresponding organization in the underlying physiological processes. These underlying processes, however, must be thought of as fundamentally molar in their nature, not molecular as in the Wundtian or Titchenerian system. The brain-processes are not considered to parallel mental elements, or to cause simple-reflex responses, but to reflect larger, better-integrated *wholes of experience* or *gestalten*, and to cause equally well-integrated totalities of action.

* "External" and "internal" are not Koffka's terms. They are used here as convenient short-cuts to avoid developing another set of distinctions in Gestalt that would not greatly enhance the value of the present sketchy account of the system.

We know about these molar physiological processes, according to Gestalt theory, just as we know about a person's direct experience, although we may not always be able to state the exact nature of the processes in a way that would satisfy the physiologist (particularly the nineteenth century physiologist). From the consideration of direct experience, together with the unconscious determinants above-mentioned, we build up our notions of these physiological events, just as, in fact, we construct the geographical (physical, "stimulus") environment. We find organization in the physiological, as well as in the physical, world because of a fundamental organization of direct experience. Only a broken-down physics and an outworn psychology could, in the opinion of Gestalt psychologists, find anything else.*

We may go even further. Since language is the symbolic mechanism in humans for telling about their organized experience, it may serve at once as a symbol of the physiological and, less directly, the physical or geographical reality: "if, to me, my language is an adequate symbol for my own direct experience, it is an objective symbol for those physiological processes at the same time" (Kohler, 1929). When there is no language, as is the case with animals, some other form of behavior will do just as well. Says Köhler: "The behavior of a chick can tell me without words that he is able to react to one brightness in relation to another. If in the course of an experiment, a human subject tells me that he sees one object as brighter than the other, the scientific value of this sentence is exactly the same as that of the chick's behavior."

One important word in Koffka's definition still awaits our attention. It is a word that indicates the major emphasis of Gestalt psychology and is almost synonymous with

*The Gestalt view that the basic, dynamic structure of consciousness is the *same* as that of the related physiological events is called "isomorphism" ("equal in form").

"gestalt" itself. This word is "field"; and Koffka is the one who has used it most. Outside of "organization," "field" is perhaps the most frequently encountered systematic term in Koffka's book.

Koffka tells us that in physics the concept of "action at a distance" of objects upon one another has been supplanted by the notion of "fields of force" in the medium between the objects; that the physicist now speaks of electromagnetic and gravitational fields and holds that the distribution of stresses and strains in the environment of an object with a given constitution will determine what that object will do. Likewise, a knowledge by the physicist of what the object does will tell him the properties of the object's field. For instance, the movements of magnetic needles tell about the magnetic field of the earth, while the movements of pendulums tell of the gravitational field.

"Can we introduce the field concept into psychology, meaning by it a system of stresses and strains which will determine real behavior?" The answer to this question is already known to the reader, who knows also, from what has gone before, just what the field will include. He may not know, however, of the dynamic stress-and-strain character which Gestalt ascribes to this psychophysical field. I shall offer, later, experimental examples that will demonstrate this character in several ways, but a few everyday observations may be taken from Koffka as particularly illustrative.

How often have we seen the beginning cyclist come to grief against the only tree or telephone pole in an otherwise vacant lot? How often does the novice at hockey or soccer drive his puck or boot his ball squarely at the body of the goal-keeper who stands stock-still between the enemy posts? More often, we say, that is accounted for by chance; and Koffka would warmly agree! He would say that there are, in a behavioral environment of the kind in these examples, "things" and "holes" between them; and the former are

more compelling and attractive than the latter. The things are centers of force, so to speak, that determine the behavior. Until the rider or the player learns better, i.e., reorganizes or reconstructs his behavioral world, he will react to the things rather than the holes.

These are examples of the dynamic quality of the behavioral field, and there are many others of a similar sort, but Koffka's point should now be clear. The reader should appreciate to a greater degree the intrinsic nature of the psychophysical field as the Gestalt psychologist treats it; and he should see why the static, bundle-concept of experience could never have appeal for the members of this school.

For the last time we turn back to the Gestalt definition of psychology, "the study of behavior in its causal connection with the psychophysical field," with the aim now to review what we have learned. We have seen that the behavior studied is to be molar (large, organized totalities) rather than molecular (small, isolated elements) and that molar behavior takes place in (is caused by) a behavioral environment—which is the geographical environment as the organism views it. We have seen that when certain other experience, of a more subjective nature, is added to this behavioral environment, we have the equivalent of direct experience or consciousness; and we have found out that, in addition to direct experience, there are "unconscious" factors that go to complete the psychophysical field. Furthermore, it appears that, wherever knowledge permits, we may substitute molar physiological processes for the direct experience of the psychophysical field; and, in any actual experiment upon human or animal subjects, we may find out about the nature of these processes from language or other behavior. Finally, we have had a glimpse of the meaning of "field" in the Gestalt system.

It is tempting to boil all this down to a one-sentence formula, stressing the view that the physical (geographical) environment causes molar, dynamic events in the nervous system that give rise in turn to molar behavior. However, this would be to neglect the direct experience that goes along with some of these physiological changes and which is, to the Gestalt psychologist, the starting-point of his science. Moreover, Koffka himself says that, although the members of this school aim one day to talk in physiological rather than experiential terms, it will be convenient for some time yet to use purely psychological language—although this language must not be that of Titchener or Wundt.

After this much of an introduction to the Gestalt system, and following the same procedure we adopted in connection with the three preceding systems, we may ask: What is the subject-matter, and what are the methods and problems of Gestalt psychology? Our answers have been given, at least in broad outline, in what has gone before, but it will do no harm to restate them.

If we choose to call the *subject-matter* "behavior," Koffka will tell us that we can know about behavior only through our own experience of it; if we choose to call it "experience," he can point out that experience—at least the experience of another person—is known only through behavior! If we say "experience *and* behavior," he can argue that we are ignoring other important events! Perhaps we had better say *behavior as determined by psychophysical processes* and hope, by thus approximating his own definition of psychology, to win his approval.

The *method* of Gestalt psychology seems at times to be that of direct observation and at times that of introspection. Yet it is not the kind of direct observation that finds reflex units, nor the kind of introspection that discovers

mental elements.* We shall see, later, with specific examples, what kind of behavioral and experiential phenomena the Gestalt psychologist encounters when he uses each method, and this will help us to understand the methods themselves.

The *problem* of Gestalt psychology is to determine the intrinsic nature and organization of the psychophysical field and to study its relation to (a) the geographical environment and (b) the behavior that results from the field-organization. It aims also to "explain" direct experience by reference to the underlying, purely physiological, field.

Already a large body of experimental data has been collected, laws have been established, and theories have been advanced that bear upon this program. Gestalt has been a productive system from the start. Although this is no place for a comprehensive treatment of these matters, a few specimens may be selected as indicative of certain interests of the school and as illustrative of its protest against the structuralistic and behavioristic points of view.

The initial experimental study of the school was, as mentioned at the beginning of this chapter, an investigation, by Wertheimer, of "apparent movement." Since no other Gestalt experiment has been referred to more often in psychological literature, a brief review of this classic research is appropriate here, even though Wertheimer's work has been greatly developed and expanded by other scientists, both German and American, in more recent studies.

If two electric lights are placed a few feet apart along one edge of a table and an upright rod is put on the other edge between the lights and a near-by wall and equidistant

* The sort of introspective observation used by Gestalt psychologists is called "phenomenological," by which is meant "as naïve and full a description of direct experience as possible"—as opposed to a kind of description "which analyzes direct experience into sensations or attributes or some other systematic, but not experiential ultimates."

from each light, two shadows of the rod may be perceived on the wall. Now, if the lights are switched on and off alternately, in rhythmical succession and at the proper rate of speed, the shadow of the rod will appear to move back and forth between its two positions. When the time between the two light exposures is too short, the shadows will appear simultaneously each in its own place; when the time is too long, there will be merely a succession of shadows—first one and then the other, in their respective places. In neither of these extreme cases will movement of the shadows appear, but various degrees of apparent back-and-forth movement of the shadows may be observed as the time-interval between the successive turnings-on of the lights approaches an optimum, which, for Wertheimer's experiment, was about sixty milliseconds (.060 sec.).*

The reader is undoubtedly familiar with things similar to this in everyday life, particularly in the compelling movement-portrayal of various electric signs where, commonly, each bulb provides a single dot of light but what one sees is a moving streak. And then there is the smooth motion of modern moving-pictures (an invention, incidentally, based upon the psychological studies of a Belgian physiologist more than a hundred years ago). However, Wertheimer was not trying to re-invent or improve upon such devices; he was interested in more fundamental scientific matters—the "How"? and the "Why"? of apparent movement.

After Wertheimer had gathered his experimental facts, he undertook to explain them. Obviously, the movement that his subjects reported was "subjective," since there was no "real" movement; the movement occurred in the "behavioral" rather than the "geographical" environment. However, there were no current explanations of the phenomenon which seemed to do justice to the actual experience. Wundt

* Wertheimer employed a more refined technique than the one outlined here, but it was the same in principle.

had argued that kinesthetic sensations, produced by the quick movements of the eyeballs as the subject glanced from one stimulus to the other, might provide the cues for movement-perception—just as Berkeley had argued that other eyeball sensations gave us our criteria for judging the *distance* of an object from us. Wertheimer, however, was able to get clear-cut reports of movement even when *two* pairs of stimuli were used simultaneously, one calling for movement in the *opposite* direction to the other, and where the time taken for the presentation of both stimulus-pairs was less than the time required for the eyeballs to react to a single pair.

Other explanations were found to be equally unsatisfactory. Wertheimer concluded that no solution of the problem could be reached in terms of simple sensations conceived to be good representations of simple physical objects (stimuli) or simple unitary neural processes. Moreover, he was not satisfied with the idea of calling the movement an illusion or attributing it to the influence of past experience or "meaning." He believed that it was as real an experience as any other and just as worthy of psychological attention—that it was an experienced *totality* and was not to be reduced to some combination of elementary sensations, held together by no matter what sort of "mental glue."

Using Koffka's terms we might say that Wertheimer's position is that the behavioral environment is not related point-for-point with either the geographical environment or the underlying physiological events. (The name given by Gestalt psychologists to that theory which *supports* a one-to-one correspondence between specific stimuli and specific sensations is the "constancy hypothesis." Wertheimer's results, of course, argued against this hypothesis, as well as the previously mentioned "bundle hypothesis.")

In order to emphasize the unique "gestalt" nature of movement observed where physically (geographically) there

was none, Wertheimer named it the "phi phenomenon" and undertook to investigate it in its own right. The explanation he arrived at was "isomorphism," referred to in an earlier footnote of this chapter. Apparent movement—a dynamic, unified *whole* of experience—was interpreted as due to a similarly organized *whole* in the brain processes. Molar phenomena were explained, that is, by reference to molar physiological events which had a structure that paralleled the mental structure in its properties. Wertheimer dismissed the traditional notions of neural function, with their stress upon the compounded activity of tiny anatomical units, and assumed instead the existence of "cross-processes" in the brain and its attached organ, the eye. Only in this way was he able to understand the perception of movement when there was no movement in the stimulus-situation itself. The perception was held to be the result of a dynamic organization within the nervous system—an organization that took place naturally under the proper conditions of stimulation.

This has been a basic type of argument in Gestalt psychology ever since. Let us take another example from the study of visual perception to make the point clearer—the problem was raised by Bishop Berkeley when he sought to explain how we perceive the distance of objects from us.

The third dimension of space, according to Berkeley, "of itself and immediately, cannot be seen. For, distance being a line directed endwise to the eye, it projects only one point in the fund [retina] of the eye, which point remains invariably the same, whether the distance be longer or shorter." He went on, as we have seen, to enumerate the various cues or criteria used by us as signs of the distance of objects, thus giving a solution to the problem that was satisfactory to psychologists for a great many years.

Gestalt questions Berkeley's answer and then denies its validity. Koffka points out that Berkeley made two false

assumptions in the statement I have quoted. First, he assumed that, since the retina of the eye is two-dimensional, our visual perception must also be two-dimensional, merely giving knowledge of up-and-down in space, or right-and-left, but not of far-and-near. Secondly, he assumed that one could know about the visual field adequately from a study of the *points* in it.

The second of these assumptions is, for Koffka, little more than the affirmation of the constancy hypothesis, which he denies on the basis of such experiments as Wertheimer's upon apparent movement. The first assumption fails to consider that the sensitive surface of the eye, which is two-dimensional, is really but a "boundary surface" of the brain, which is *three*-dimensional. Why, then, Koffka asks, could not this three-dimensional brain be related *naturally* to three-dimensional experience? He concludes that it could, and offers a number of experimental observations to show that three-dimensional space is even *more* naturally perceived than two-dimensional space, that the perception of a *surface* (which is what Berkeley started with) is really a later development in visual experience than that of depth.

Koffka does not appeal, as Watson might have, to the genetic development of space-perception in humans, but he brings forth evidence from the reactions of adults to various portrayals of three-dimensional objects (e.g., the picture of a wire framework in the form of a cube), which is taken to show that the appreciation of depth is really a more primitive form of organization than surface-perception. Moreover, he uses other experiments to show how various stimulus-conditions, including the cues or criteria of the older psychology, might be used to help or to hinder this natural function.

A single example of these experiments will illustrate the ingenuity of the Gestalt attack. Suppose that under the same circumstances in which apparent movement is re-

ported, there be presented, in rapid alternation, a v-shaped figure and, directly above it, an inverted-v (so placed that, if both were presented at once, a not-quite-closed diamond would be seen). What kind of movement will result, under optimal conditions? Will there be movement of an up-and-down nature, causing a distortion of the figure in a two-dimensional plane; will there be some sort of motion around the vertical axis within the plane of the drawing; or will there be a rotation in the third dimension around the horizontal axis?

The reader is predisposed, by the way in which the problem has been stated, to reply that the third possibility is the most likely one; and he is right, although the other two types of movement occasionally occur. The most common report is that of a wheel-like rotation around the horizontal axis—a rotation in the third dimension of space! In other words, third-dimensional movement appears in the behavioral environment when there is no movement, not even two-dimensional, in the geographical.

Experiments of this sort have been very puzzling to those psychologists who have followed in the Berkeleian tradition and tried to show how depth-perception is built up in a spatial world that has little or no depth to begin with. Moreover, such experiments possess a broader significance than that which bears upon this particular problem. They illustrate the manner in which old problems, even "dead" ones, are often revived in science with the application of new methods and new points of view. However much, or little, Gestalt has done in re-defining psychology, the fact remains that the viewpoint has been experimentally refreshing and productive.

My two examples of Gestalt research have been chosen from the studies made upon the visual portion of the behavioral environment. The most original and provocative, as well as the greatest number, of this school's investigations

have been made in the field of visual perception. Yet it should not be concluded that the other senses have been entirely neglected or other chapter-headings ignored by the members of this group. Gestalt aims to throw light into every hidden recess of psychological thought and to demonstrate the value of applying Gestalt principles to every type of psychological function Koffka's book, to which I have referred so often, contains, in addition to chapters on the "environmental field" of the organism, chapters on "action," "memory," and "learning," as well as a chapter on "society and personality" with which he ends his lengthy exposition. Everywhere the treatment is permeated with the same point of view; everywhere *organization* is stressed, whether it is organization of the visual field, of the "Ego," or of the neural traces underlying memory. Everywhere, in the examination of experience or behavior, gestalten appear—i.e., organized totalities of experience or behavior which have definite properties not traceable to parts and their relations.

The most characteristic theme of Gestalt psychology, that the whole is more significant than, and determines the nature of, the parts that compose it, is brought out in ever so many contexts within Gestalt literature. Even the colors of everyday objects, the notes of a heard melody, the sequences of habitual or instinctive action are shown to be dependent upon the spatial or temporal structure of which they are parts. For example, a white paper in deep shadow still seems white and a black paper under direct illumination holds its blackness, in spite of the fact that the actual intensity of light reaching the eye from the two surfaces may be the *same*.* Likewise, coal looks black in the sunlight when the (geographical) stimulus-energies ought to make it look

* A simple test of this sameness, offered years ago by Helmholtz, may be made by looking at the two surfaces through a peephole in a gray cardboard—thus excluding the "normal" surroundings of the papers. Under such conditions the approximate equality of the brightnesses appears.

white; and a plate in the middle of a dinner-table still appears round although more often seen otherwise, i.e., elliptical. A dancer may execute a difficult double-shuffle, yet be unable to demonstrate the parts of the step; and an ape may portray the hunger instinct with never the same muscular components. Gestalt consideration of the "whole" has even provided some justification for judging character from handwriting and photographs—procedures long since discredited by the elementaristic type of research that dealt with measurements of letters and other details and with isolated facial features.

There are many more concepts and configurational (gestalt) laws that have been developed by the members of this school, particularly in their treatment of perceptual matters; but these belong to a more detailed account of the system than this one pretends to be. A few additions to the picture may, however, be made in considering some of the charges brought against the system by its critics.

The indictments of Gestalt have been numerous, but they have been, in general, more academic in their nature than those of Watson's behaviorism. Thev have, so to speak, been mostly "intramural"—in spite of the fact that Gestalt professes to be closer to everyday and common-sense reality than either behaviorism or structuralism. Whether this is due to agreement with, or ignorance of, Gestalt precepts on the part of the general public in Germany or America may be a question, but it is likely that the latter is the case. In spite of the almost religious zeal of some Gestalt psychologists, and notwithstanding a dramatic character which their studies sometimes take on in class-room presentation, the problems and experimental interests of the school are of too technical a nature to arouse the widespread discussion which is stirred up by Watsonian pronouncements.

Gestalt psychology has been accused of denying its ancestors and ignoring its contemporaries—of failing to recog-

nize the historical roots of Gestalt doctrine and of attacking views, outmoded since the days of James Mill, to which no up-to-date structuralist or behaviorist would subscribe; of substituting vague new terms for well-defined old ones; of supplanting one form of elementarism with another; of accepting the "form" and rejecting the "content" of behavior and experience; and of assuming that "organization" is its own explanation.

These are not all of the complaints that have been made, but they illustrate the range of the fault-finding, not all of which has abstained from open ridicule. Witness such titles as *The Phantom of Gestalt* and *The Little German Band.* Some of this criticism has severely tested the recognized power of Kohler and Koffka in debate, and deserves a few words before our discussion is ended.

It has been stated earlier in this book that we are bound to *analyze* whenever we describe an object or an event; yet we learned later that Gestalt psychology began with a protest against analysis, at least analysis of the Titchenerian stripe. Now we see that this school has been criticized on the ground that it substitutes one kind of analysis into elements for another. The fact of the matter seems to be that in their earlier attacks upon other schools the Gestalt theorists somewhat overstated their position. Kohler, more recently, has denied that the system has no place for analysis, but he contends that Gestalt analysis finds more natural (molar) units than those of structuralism or behaviorism. He admits, moreover, the possibility of more artificial (molecular) analysis itself, as long as it is appreciated that the elements arrived at are not really observable in experience or behavior.

With the recognition of these elements the question of the validity of some part of the associationistic doctrine naturally arises; for we of the period since John Locke are bound to ask about the nature of the connection between

elements. Titchener gave us a fundamental "law" of association, and Watson prescribed "conditioning"; what have Köhler and Koffka to say? An answer to this question might easily lead us into a discussion of memory, learning, thinking, and related matters—for which we have no space. It must suffice to note that the school *does* espouse a tempered and restricted form of associationism, one that deals with organized neural traces and the "communications" between them. Apparently the doctrine against which Gestalt pits itself is one that arises from a bad choice of elements and an imperfect, reflex-arc neurology.

We may now close the book of Gestalt. It has been a difficult one to read (or reread!) but it may have been worth the candle, for Gestalt is one of the "last words" in systematic psychology. Although it is not the only school in Germany to-day, it is at least the outstanding one, and its influence has been great in America as well. The intrinsic merits of the system; the novelty and significance of its experimental support; the inspired and able missionary work of Köhler, Koffka, and others—all these factors have led to widespread recognition of the movement. The very "gestalt-character" of the school—its dynamic, purposeful organization, with its "whole" more important than its "parts" or members—seems to have been responsible for its unique position in modern psychology.

Like the structuralism of Titchener, Gestalt has been consistent and self-contained; like the functionalism of Chicago, it has been flexible and broad; like Watsonian behaviorism, it has been healthy and aggressive. Yet, with all three, it will probably be absorbed within an even more consistent, flexible, and enduring point of view—one that may not wave a banner or sing a battle-hymn, but one that will comprise the best of what has gone before and will be known, not as a *system*, but as *psychology*.

Chapter VII

THE PROBLEM OF DEFINITION

IT has already been stated: "a system of psychology is, in a sense, nothing more than a logical framework into which may be fitted the findings of the science." In another sense a system is nothing more than an elaborate *definition* of psychology. Every textbook is a step-by-step vindication of a point of view announced, if the author is truly "systematic," in an introductory chapter, paragraph, or sentence. Only when we have carefully examined the system, through reading of the text, are we able to grasp the substance of the definition.

Titchener told us that psychology was "the science of the mind," but it took many pages to make his meaning clear. The same was true of Carr's "study of mental activity," Watson's "behavior," and Koffka's "behaviour in its causal connection with the psychophysical field." All of these preliminary definitions required much working-over and expansion, although each of them pointed with some directness to a particular creed that was to be developed.

There are more definitions of psychology than the four we have considered—hence, more systems. "Hormic" psychology, "dynamic" psychology, "act" psychology, "reaction" psychology, "understanding" psychology, and "reflexology"—all have been claimants of attention. Each has presented a brief and has called up its witnesses; each has offered a different plea; but none has received such a favorable verdict in America as those we have discussed.

The modern tendency in psychology is, nevertheless, away

from the multiplicity of systems and towards a unification. In fact, had I not been so much interested in the "genetic" aspect of our problem of definition, *two* schools might have been reviewed here rather than four; and I might have gone further, to reduce the list to *one*. Psychologies are gradually giving way to psychology, and the era of schools may soon be ended.

The present treatment, it is true, has stressed the differences between systems. I have contrasted the structuralism of Titchener with the behaviorism of Watson, and measured both against Gestalt. The principal warrant for such a procedure was historical—to provide a developmental picture of our science. Yet it must now be confessed that this method was unjust and incomplete; it failed to emphasize similarities and it omitted certain phases of systematic progress.

Watson's behavioristic views are as clearly set off from present-day behaviorism as they are from Gestalt; Titchener's structuralism of 1915 was not his structuralism (existentialism) of 1927 and is certainly not to be likened to the systematic positions of his pupils; and the functionalism of Harvey Carr has undergone modification since 1925.

If we were open-mindedly to compare Koffka's postulations with those of such system-conscious modern leaders as Tolman, Boring, and Hunter, we would find, in spite of divergent interests, of annoying differences in terminology, and of disagreement on one or two major issues, a remarkable similarity—a similarity that grows with every year and is often as great between schools as it is within them. We would see one concept after another taking on the meaning of its rival; we would hear behaviorists talking in the manner, if not with the words, of Carr, and structuralists speaking the language of Gestalt. But it would also be quite apparent that a single unanimously accepted definition of our science is not at hand—yet.

We have taken the easier road, but have gone almost as far. We have discussed but one truly modern viewpoint —that of Gestalt—but the discussion of others might have brought us no closer to our goal. At the end the reader might have asked, as he may ask now: Why don't these systematists get together? Who is right and who is wrong? or even, What of it? These questions would have been, and are, fairly put, and they deserve at least an attempt at an answer.

A science-in-the-making is not, in practice, an abstract, impersonal affair. It comprises a not-entirely-coöperative group of human beings, each one of whom has his own background of knowledge and research, his own peculiar capacity, his own prejudices and preferences—in a word, his own individuality. Disagreement and controversy are, therefore, almost inevitable, even with respect to very fundamental questions. The textbook "laws" of every science, no matter how sweeping in their significance, are, at best, generalizations with which the majority of scientists will agree. Without some dissension, indeed, scientific progress could not be so great as it is.

This is especially true of a young science like our own —newly set free from philosophy and not always distinguishable, in some of its labors, from physiology—and it is not strange that there should be a difference of opinion today, even with respect to the problem of definition. The astonishing fact is that there is so much accord on systematic matters.

The first of the queries noted above—why systematists disagree—is thus answered: psychologists are human, and psychology is a young science. We may now take up the second question, rephrased: What is to be the true definition of psychology?

Obviously, from what has already been said, a simple, direct, and satisfactory answer cannot be given if I am to

remain true to my identification of definition with system; and this is not the place to present a detailed development of a single point of view. On the other hand, it *is* possible to indicate, in a general way, the shape that psychology seems to be taking in the hands of modern workers. Present-day psychology may at least be characterized by reference to certain rather prominent earmarks, as these show themselves in American interests and teachings.

(1) Psychology is becoming more *objective* in its point of view. This may readily be seen in most of to-day's text-book definitions, which emphasize *behavior* rather than experience. Even Koffka "starts with" behavior and only "finds a place for" consciousness. It may also be seen in the widespread tendency to minimize the importance and point to the unreliability of the introspective method—a tendency that may have overshot its mark, since the altered interpretation of the method, on the part of its strongest adherents, makes it less "subjective." When Titchener included "report" in the introspective formula, he paved the way for a behavioral definition, and Köhler seems to do likewise when he accepts language, behavior, as an adequate symbol for direct experience.

The modern theorist tends more and more to think of the various categories of experience—sensations, images, feelings, gestalten, and mental activity—as *inferential* rather than *observable* entities. Especially in experimental research does it appear that the subject's mind is inferred from his behavior or accomplishment. For example, when a subject reports a shift of hues under changing stimulation, it is *assumed* by the experimenter that experience was there, although he cannot perceive it.

(2) If the modern psychologist is less concerned with consciousness, he is also less concerned with *physiology*. Notwithstanding the fact that physiology is still the handmaiden of psychology and is still used by many to explain

psychological facts, it is apparent that our science stands more upon its own feet than it ever did before. Research workers are more content to describe their findings in terms of the conditions under which they were obtained—the actual experimental procedures employed—and less hasty in appealing to neural events that are no less inferential than the consciousness they may be supposed to parallel. It is beginning to be realized that the physiologist who "describes" the course of a nervous impulse from sense-organ to muscle, on the basis of the twitching of a dog's hind leg in response to the scratching of his back, is on no firmer *observational* ground than the psychologist who "describes" the pain of a subject who jumps at the prick of a pin. The only advantage possessed by the physiologist in such a case is the one of working in the same universe of discourse with the physicist and the chemist—of being able to talk in physico-chemical terms rather than purely "psychical" ones. There may be, as Köhler has said, a great deal of *terra incognita* between the *stimulus* and the *response*, but not all psychologists are as eager to explore it as he appears to be, and some are quite willing to deal first with territory that is nearer home—namely, the measurable relationships between the two end-terms.

(3) Psychology pays more attention than formerly to what I have called the *durational* aspect of human activity. Whether the structuralist's "determining tendency," the functionalist's "adaptive behavior," the behaviorist's "chain of reflexes," or Gestalt's "temporal organizations"—or any substitutes for these—are employed, there is no longer a neglect of the fact that human behavior, or experience, is more often than not to be treated in terms of a describable beginning, an appreciable duration, and a definite end. "Interests," "instincts," "urges," "insight," and "drives" are frequently encountered in up-to-date texts—each concept trying to shoulder some part of the burden. It is increas-

ingly apparent that even such everyday activities as the mailing of a letter, the telling of a joke, the solving of a problem, or the eating of a meal cannot be understood without this durational view.

(4) Psychology is more *genetic* and *comparative* than it was in Titchener's day. The notions that the child is father to the man and that man is kin to the brute may have waited a long time for psychological appreciation, but each is now part and parcel of our explanatory scheme. The adult human being is not the only focal point of interest. Most psychologists feel that there is much to be gained from a study of the genesis of man's activity and a comparison of human with animal functions. The tremendous development, in recent years, of the fields of animal and child psychology provides many illustrations of this change in outlook.

(5) Related to the above-mentioned shift in emphasis is the tendency of the modern psychologist to investigate the activity of other than the average normal human subject. Studies of *individual differences,* including those extreme psychological deviations which we call *abnormal,* are illustrative of this. Besides the generalized "mind" of early laboratory research, the systematist of to-day must find a place for particular "minds" that show differences of great or less degree. A variety of methods—experimental, statistical, and clinical—have been brought to bear upon these problems and have given to this type of inquiry a dignity and objectivity that may no longer be questioned or disregarded by the "pure scientist."

In this list of characteristics a strong behavioristic influence may be noted. It is conceivable that a more adequate list might have pointed oftener to other schools. However, the really important determiners of one's definition of psychology lie in one's judgment of the true subject-matter, the predominant methods, and the unique problems of the

science. In all three respects, if the above analysis is correct, behaviorism leads the field in American psychology.

This is not to deny the influence of other schools or to pretend that all American psychologists profess to be behaviorists. Much modern doctrine is a direct outgrowth of functionalism; some of the best in it is a remodeled and refined product of structuralism; and a growing share may be attributed to the productive, although not necessarily the decrying, aspect of Gestalt.

The majority of psychologists to-day would probably be unwilling to give wholehearted allegiance to any one system. Currently there is a tendency away from adherence to, or the development of, a single viewpoint, and towards the acceptance of as many insights as the nature of specific research problems may be thought to require. It remains to be seen whether this eclectic bent—this picking and choosing of concepts from a variety of systematic programs—will lead to muddlement and confusion or to better-informed and more comprehensive system-making. It is quite certain, however, that the systematist of to-morrow will be harder pressed than ever to keep the increasing bulk of fact within the framework of his theory.

The third and last question relating to systematic psychology and the problem of definition was the skeptic's challenge: What of it? This may be taken to mean, as it usually does, What is the value of a system? If the answer is not already clear from what has gone before, a few concluding remarks may help to make it so.

A system is a sincere and thoughtful attempt to keep the house of psychology in shape: to replace this bit of furnishing and to repair that; to rearrange these articles and to throw out those which serve no purpose; to indicate the out-of-date appearance of one room or the bareness of another; to add a wing here or a storey there; and, if necessary, to tear down the house and build anew—all in keeping

with the number, the needs, and the possessions of its occupants.

The analogy is tempting, and I might easily go further, but perhaps the point is made. A psychological system is an attempt to arrange and coördinate, in a logical and understandable fashion, the facts of the science into a meaningful and satisfying whole; to point to the weaknesses and gaps in our knowledge; and to show the way to future achievement. In addition, a system is an attempt to say just what psychology *is*, what it is the science of, what the whole enterprise is about—to define a subject-matter and hence to direct research.

Evidence for the value of systematic psychology is so common as to go often unremarked. Imagine, for a moment, an elementary textbook of human psychology. Why does it have the form that it does? Why are various facts and laws placed in one chapter, under one heading, rather than another? Why does this chapter precede or follow that one; why, indeed, are not the facts treated as in the index—alphabetically? No diagram is needed to convey the answer: the text, willynilly, is written from a point of view, and some semblance of system is bound to be there; and the more of true system, the less of mere cataloguing.

In any textbook both content and form demonstrate system-value. Facts do not choose their system—the reverse is more often the case—and they frequently owe their very existence to some systematic point of view. Titchener may have been too narrow and functionalism too broad, Watson too naïve and Gestalt too intent upon slaughtering ghosts; but each may be given credit for a portion, large or small, of the basic framework of our science.

REFERENCES

In connection with the preceding chapters there are a number of books that may be recommended to the student who desires further acquaintance with this problem of defining psychology.

Edna Heidbreder's *Seven Psychologies* (published by The Century Co.* in 1933) is the most logical "follow-up" to our discussion. This excellent book contains, in very readable form, a more comprehensive historical account than our approach has provided, together with a more inclusive review of the schools.

Robert S. Woodworth's *Contemporary Schools of Psychology* (The Ronald Press Company, 1931) is a less complete résumé of the systems, but may profitably be read in conjunction with, or after, Heidbreder's book.

For those students who would like first-hand knowledge of the different points of view and wish to increase their store of facts and theories, it is of course advisable to go directly to the systematic texts themselves. E. B. Titchener's *A Beginner's Psychology* (The Macmillan Company, 1915) has already been mentioned as a good statement of the structuralistic view; Harvey Carr's *Psychology* (Longmans, Green and Company, 1925) is perhaps as near to being a functionalistic text as any—especially in its treatment of such concepts as that of "adaptive behavior" and the "reflex arc"; and John B. Watson's most scholarly outline of his position is in his *Psychology from the Standpoint of a Behaviorist* (J. B. Lippincott Company, 1924, 2nd edition),

* This book is now published by D. Appleton-Century Company.

although a more popular presentation is to be found in his *Behaviorism* (W. W. Norton and Company, 1930, revised edition). Neither Wolfgang Köhler, in his *Gestalt Psychology* (Liveright Publishing Corporation, 1929), nor Kurt Koffka, in his *Principles of Gestalt Psychology* (Harcourt, Brace and Company, 1935), has given a picture of Gestalt that is very well suited for undergraduate contemplation. G. W. Hartmann's *Gestalt Psychology* (The Ronald Press Company, 1935) is much more so.

There are several fairly recent "histories" of psychology for the reader who wants to go further in this direction. Among them are E. G. Boring's *A History of Experimental Psychology* (The Century Co.,* 1929), Gardner Murphy's *An Historical Introduction to Modern Psychology* (Harcourt, Brace and Company, 1929), W. B. Pillsbury's *The History of Psychology* (W. W. Norton and Company, 1929), J. C. Flugel's *A Hundred Years of Psychology* (The Macmillan Company, 1933), and W. S. Hulin's *A Short History of Psychology* (Henry Holt and Company, 1934). Boring's is the most systematic; Murphy's is somewhat more comprehensive in its scope; Pillsbury's provides many interesting sketches of the lives and contributions of psychology's fathers; and Flugel's and Hulin's are probably best for the beginner.

* This book is now published by D. Appleton-Century Company.

Index of Subjects